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THE FORMULATION AND ELABORATION OF THOUGHT-PROBLEMS

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We commonly accept with uncritical complacence man's claim to superiority over the 'unthinking' animals. Psychologists themselves have long recognized this claim by the phrase 'higher mental processes' to be accorded to human beings. The phrase implies a hierarchy among man's abilities, with senses and instincts near the lowest level and thinking and reasoning at the top. Despite the attempt of certain behaviorists to reduce thinking to the stumbling processes of trial-and-error or to mere movements of mouth and throat, as well as the proposal of others to call 'reasoning' any kind of solution achieved through any means by any animal confronted by an experimenter's problem, there has remained the conviction that human accomplishment and human advancement have rested, in large part, upon certain specific—if not 'higher'—operations of real thinking.

Since thinking and its fruits are of great importance, it is surprising to discover that underlying activities of the organism have been consistently slighted in the laboratory. The difficulties of the topic are in part responsible; but less responsible, as it appears, than the success in human practice of easy substitutes for thinking. Most men avoid thinking, which requires labor and pains, readily accepting these easy substitutes. In his *Science et méthode,* Poincaré remarked thirty years ago that "la plupart des hommes n'aiment pas à penser et c'est peut-etre un bien, puisque l'instinct les guide, et le plus souvent mieux que la raison ne guiderait une pure intelligence . . . It faut donc penser pour ceux qui n'aiment pas à penser." Though we were forced to agree that thinking is not for all, we should not relinquish either the firm conviction that skilful and successful thinking by the capable

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is of primary import or the contention that the operations of thinking still present a challenge to the descriptive psychologist. The two main aspects of this complex operation; namely, formulation and elaboration, are named in our title.

Most of the experimental investigations at hand have been limited to the securing of estimates, in quantitative form, of the *product* or *issue* of something called thinking but wanting a description or so much as a clear definition of the means and method of production; without, that is to say, first coming to an agreement or understanding upon the actual nature of the alleged performance itself. These studies have been concerned with the number of problems solved, promptness or delay in solution, the relation of failures to successes, the retention of completed and incomplete tasks, and the relation of outcome to such abstractions as 'intelligence' or 'drive.' The common method has been loosely to include under 'thinking' and 'reasoning' an animal's (including the hominid's) 'successful' behavior in a situation which seemed to the experimenter to be 'problematical.' Here the experimenter's double interest in the rat's behavior and in his own 'problem' tends to blind him to the logical confusion involved in relating the one to the other. For it should be clear that the significance of *the subject's solution* lies in its relation to *the subject's problem.* There is a touch of absurdity in an experiment upon problem-solving in which a rat, a dog, or an infant solves a man's problem.

The sparseness of descriptive material on the nature of thinking itself is reflected in current textbook definitions. Here are samples.

Thinking defined as 'association of ideas.' Warren and Carmichael may be selected as representative of those who identify thinking with association.[1]

"In man imagery, and more especially such compounds of imagery as make up thought, tend to become one of the most important phases of mental life. A perception arouses a thought, this thought excites another thought, this in turn a third, and so on. A long series of thoughts may arise in quick succession. . . . Such a train of thoughts is called thinking. . . ."

"For example, you hear a certain humming noise and think of an airplane. This suggests the thought of the airmail, then you think of crossing the Atlantic, then of a great Atlantic liner. . . ."

"The succession of mental images and thoughts is commonly called *association of ideas.*"

Thinking defined as 'subvocal behavior.' The strictly behavioristic treatment is best exemplified by Watson, who defines thinking in terms of subvocal behavior.[2]

[1] H. C. Warren and L. Carmichael, *Elements of Human Psychology,* (rev. ed.), 1930, 298.

[2] J. B. Watson, *Psychology from the Standpoint of a Behaviorist,* (3d ed.), 1929, 356.

"Thinking would comprise . . . the subvocal use of any language or related material whatever, such as the implicit repetition of poetry, day dreaming, rephrasing word processes in logical terms, running over the day's events verbally, as well as implicit planning for the morrow and the verbal working out of difficult life situations. The term 'verbal' here must be made broad enough to cover processes substitutable for verbal activity, such as the shrug of the shoulder and the lifting of the brows. It must embrace the implicit movements demanded in the use of the deaf-and-dumb sign manual, which are, in essence, word activity. Thinking then might become our general term to cover all subvocal behavior."

Thinking defined as 'mental trial-and-error.' The weakness of the behavioristic view resulted in numerous modifications which sought to account for the formerly denied 'mental' factors in thinking. Of these modifications, the most tenaciously held defines thinking as 'mental trial-and-error.' As interpreted by Woodworth and by Murphy, for example, the view relates thinking to *remembering.*

"Reasoning may be described as mental exploration. Suppose you need the hammer, and go to the place where it is kept, only to find it gone. Now if you simply proceed to look here and there, ransacking the house without any plan, that would be motor exploration. But if, finding this trial and error procedure to be hopeless, you sit down and think, 'Where can that hammer be? Probably where I used it last!' You may recall using it in a certain place, go and find it there. You have substituted mental for motor exploration, and saved time and effort."

"The *process* of reasoning is illustrated very well in this simple case. In a way, it is a trial and error process. If you don't ransack the house, at least you ransack your memory. . . ."

"It differs from mere trial and error in that it follows clues by thinking them through, instead of by motor exploration. It follows them by recall of previously observed facts."[3]

"In its *simplest form,* thinking may be regarded as controlled association. . . . Thinking of this sort is involved in the effort to recall a name which is on the tip of one's tongue, the attempt to place a familiar face for which no name is found. . . . Thinking consists of searching for the right answer to a question. . . ."[4]

Thinking defined in terms of everyday usage. This convenient, though undiscriminating, usage can only serve to convince the beginning student that psychology is merely 'common sense.' The instructor who seeks to break down this false conviction will find the definition a hindrance rather than an aid.

"What do we mean, in psychology, by 'perceiving,' 'thinking,' and 'feeling?' We mean nothing more, and nothing less, than what is meant by them in the every-day conversation of men of average education. These are terms which, by the conventions of language, are employed to designate occurrences which are recognized by all. By majority usage, the general term which covers 'remembering,' 'imagining,' and 'anticipating,' is *thinking.*"[5]

A few experimental studies have yielded somewhat more pertinent results.

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[3] R. S. Woodworth, *Psychology,* (3d ed.), 1934, 464-465.
[4] G. Murphy, *A Briefer General Psychology,* 1935, 314-315, 341.
[5] K. Dunlap, *Civilized Life,* 1935, 15.

Heidbreder distinguishes the products of thinking from those of other psychological performance; although she discovers in thinking no distinctive operation. She concludes that thinking is merely a composite of other psychological activities, somehow yielding its own products. Thus "thinking . . . denotes any activity which, by some means other than overt motor reaction, produces responses which are adequate, novel, and invented."[6]

Duncker advanced toward a depiction of the thinking performance, but he was hampered by the doubtful and ambiguous concept of 'insight."[7]

"Thinking is the process which by insight into a problematic situation leads to appropriate responses. . . . After the problematic situation as such has been fully comprehended, the thinking process sets in with the 'penetration into the conflicting circumstances of the problematical situation.' The 'penetration' is the first and principal phase in the thinking process. It consists in the insightful grasp of those traits . . . which cause the conflict."

Approaching the subject from a new direction, Ryan and Lemmon have given actual descriptions of certain phases of thinking operations, Ryan[8] dwelling upon the place of *the task* in these operations, and Lemmon[9] discovering the functional factors in the analogical varieties of thinking.

Further research on this topic is one of the greatest needs in psychology. As we see it, the problem is three-fold: to find the salient characteristics of thinking at large; to delimit and to describe the main stages in the total course of this operation; and to determine those means and methods of initiation and guidance by which a thinking performance progresses toward a conclusion. It is the aim of this study to discover some of the essential phases of this problem and to indicate the lines along which future research may profitably be directed. We shall try to avoid the implication of a 'mental process' which is *thinking*, and of a 'mental object' which is *thought*.

The Experiments

Three considerations were dominant in planning the experiments. (1) Materials were sought which would provoke a performance involving the integration of thinking with acting.[10] (2) The experimental occasion was

[6] E. Heidbreder, An experimental study of thinking, *Arch. Psychol.*, 11, 1924, (no. 73), 43.

[7] K. Duncker, A qualitative (experimental and theoretical) study of productive thinking (solving of comprehensible problems), *Ped. Sem.*, 33, 1926, 702-703.

[8] T. A. Ryan, Mathematical objects and symbolizing, this JOURNAL, 51, 1938, 283-303.

[9] M. L. Lemmon, A psychological consideration of analogy, *ibid.*, 51, 1938, 304-356.

[10] The value of such an approach to the problem of thinking was first indicated by Ach in his pioneering experiments (*Ueber die Willenstätigkeit und das Denken*, 1905). Although preoccupation with the search for 'thought elements' and the ready

relieved, so far as possible, from the restriction of those formal instructions which would have provided *O* with a problem designed by *E*. *O* was left free to formulate and direct the specific course of his own elaborative performances. It was thus possible to secure a description of the activities involved in formulation and also to determine whether these activities are a part of the thinking performance itself or merely an external and initiating factor. Finally (3) the experiments were devised with the purpose of avoiding so far as possible the confusions and complications arising from too great a dependence upon linguistic tools.

In brief, our procedure provided competent *O*s with an experimental occasion and preserved in uniform terminology a descriptive account of the resulting performance.[11] *E* supplemented the reports by the observed behavior of *O* during the experimental session. Since the emphasis of the study is upon a *description of functional course* rather than upon the degree of achievement, quantitative aspects of performance were of incidental interest.

Materials. There were three series of experimental occasions. The formal instruction in Series 1 and 2 was to *create and solve a problem.* When functioning took a persistently digressive course (*i.e.* away from the formulation of any problem appropriate to the occasion), a secondary instruction was interposed to create and solve a *different* problem. Before experimenting began, *O* was informally encouraged to discard the 'testing' attitude. Materials were either placed on a table before which he was seated or presented in some other visual way. To eliminate distraction, *E* was seated behind a screen. A mirror was so arranged that *E* might observe inconspicuously *O*'s behavior while at work.

Series 1. Each of the six sets of this series involved the presentation of a group of irregularly-cut pieces of cloth of various shapes, colors, and materials. The groups differed from one another in the total number and variety of pieces, and were similar in that each contained within it a sub-group distinguished by the common characteristic of thickness. The quality of thickness was indicative of a more fundamental distinction, by which the pieces of this sub-group might be set apart from the other pieces, for each of the thick pieces was part of a paper pennant disguised by a cloth cover.

All the pieces of a pennant were present in a single group. Upon discovery of this circumstance it was possible to fit them together into the whole pennant. The group presented to casual observation merely a heterogeneous collection of cloth pieces. We describe the six sets of this series.

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retreat to physiological explanation of obscure psychological performance clouded the interpretations of Ach's work, as well as that of his Würzburg colleagues, the discovery of the importance of the *Aufgabe* and of the central fact of *determination* was undeniably valuable.

[11] Observers were Dr. T. A. Ryan (*Ry*), Instructor in psychology, Dr. M. S. Ryan (*Sh*), R. M. Lindner (*Li*), and J. I. Lacey (*La*), assistants in psychology.

For the discussion of the nature of our experimental technique see M. Bentley, The nature and uses of experiment in psychology, this JOURNAL, 50, 1937, 452-469.

Emblem of	Total no.	Pennant pieces	Cloth pieces
(1) Brown Univ.	56	14 various shapes and sizes covered with figured silk	42 various shapes, sizes, colors and materials, some the same as the pennant covers
(2) Fordham	20	6 various shapes and sizes covered with the same brown cloth	14 various shapes, sizes, colors and materials
(3) West Point	36	9 various triangular shapes slightly different in size covered with figured silk	27 all of linen of three shapes and nine colors
(4) Pennsylvania	27	18 various shapes and sizes covered with material different in color and texture	9 all of silk taffeta of the same size and oblong shape but different in color
(5) Yale	12	6 various shapes of about the same size covered with plaid taffeta	6 all of the same size and oblong shape in white piqué differing in weave
(6) New York Univ.	36	12 various shapes and sizes covered with material different in color and texture	24 various shapes, sizes, colors and materials, some the same as the pennant covers and some backed with paper

Series 2. This series consisted of eight sentences in code, constructed by intermingling, in a specific pattern, the letters forming the sentence and 'irrelevant' letters. A simple example would be 'Time flies,' written 'Taibmce fdleifegs' by inserting the irrelevant series a, b, c, d, etc. The situation could, therefore, be grossly factored into two parts; the hidden sentence and the irrelevant letters. It was thus comparable to the situation in *Series 1.* There was in both a combination of the relevant and the irrelevant which could be disentangled only by close and critical scrutiny. Though greatly simplified, the situations were fundamentally similar to complicated problems presented to the investigator in the sciences who succeeds in ordering and classifying a heterogeneous mass of apparently unrelated items by the discovery of a principle through which items relevant to a problem are distinguished from irrelevant items.[23]

To replace the corrective secondary instruction given by *E* to halt digression in *Series 1,* there was substituted in the present series a succession of 'occasional' clues, hinting at the nature of the problem and of its solution. Clues became progressively more revealing and were presented at *E*'s discretion whenever performance seemed to have gone entirely astray.

The eight problem-sentences for the eight sections are given below, together with their clues, translation and mode of construction.

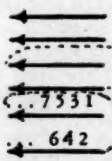
[23] The writer plans to extend this investigation to include more and more complicated problems to match really difficult thought-tasks of the researcher and the scholar. It was considered desirable to begin the investigation with the simpler forms.

Coded Form

(1) Cahbocodseef aglhtiejrknlamtnionpgq lrestttuevrwsx.
(2) Aalbtcedrenfagthei ljektltmenrosp fqorrsmt wuovrwdxsy.
(3) Dablutrezrmnwaqtae jlxepthtnecrbs vakrse liwrerieglnepvyaonft.
(4) Aaa mbboccdddieefffiggchhaiitjjikkollnmm onnfoo tpphqqerr pssrttiuunvvcwwixxpyylzzeaa abbpccpddleeiffeggshh hiiejjrkkell.
(5) ravebwvcxedyrezsfaegbdhciidsjeskfelgnmhtnieojnpkcqlermtsnhtoeup
(6) ddeetstriemvoesrienconiestsneersgeohrtpfloascriettetbealhephltadenhat
(7) tcbaoserritbkr.dawcaowhtwdawofnoinitnimoeh
(8) gsrh ziizmtvnvmg rmeloevh hfyhgrgfgrlm

Clues

(The numeral designates the sentence to which the clues apply, the letter the order in which the clues were presented. In each case the clue was typed below the problem sentence.)

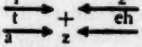
(1) a. abcde . . .
b. a b c d e f g h i j k l m n o p q r s t u v w x y
(2) a. same as 1a
b. a b c d e f g h i j k l m n o p q r s t u v w x y
c. *Aalbtcedrenfagthei ljektltmenrosp fqorrsmt wuovrwdxsy.* (Italic letters were crossed out.)
(3) a. Dburz . . .
b. D b u r z m w q a j x p h n c b v k s l w e i g n p y o f
c. *Dablutrezrmnwaqtae jlxepthtnecrbs vakrse liwrerieglnepvyaonft.* (Italic letters were crossed out.)
(4) a. aa bb cc dd ee ff . . . . .
b. Aaa mbbocc (etc.)
(5) a. abcde . . .
vwxyz . . .
b. a b c d e f g h i j k l m n o p q r s t u v w x y z
(6) a.

b.

c.

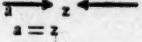
d. d e t t i m o s i n o i s s e r g o r p l a c i t e b a h p l a e h t
e. d e s r e v e r e c n e t n e s e h t f o s r e t t e l e h t d n a
f. (d and e combined, one under the other)

(7) a.
b.

c.



(8) a.



b. a = z
c. a = z
b = y
c = x
d. (same as c except that the alphabets are completed)

Translation

(1) Choose alternating letters
(2) Alternate letters form words
(3) Alternate letters are irrelevant
(4) A modification of the principle applies here
(5) The sentence is reversed
(6) The alphabetical progression is omitted and the letters of the sentence reversed

(7) The combination is one forward with two backward
(8) This arrangement involves substitution

PLAN OF CONSTRUCTION

(1) The letters of the alphabet alternate with letters forming words of a sentence. The alphabetical sequence begins with the second letter. The group begins with a capital and ends with a period (thus indicating a sentence-structure).
(2) Same as 1.
(3) Letters forming words of a sentence alternate with 'nonsense' letters. The group begins with a capital and ends with a period.
(4) Every third letter represents the sentence-sequence, between which is interspersed an alphabetical sequence in pairs. The group begins with a capital and ends with a period.
(5) Every third letter, in reverse direction, represents the sentence-sequence. The first of the pairs of letters interspersed combines two alphabetical progressions. Beginning with the second letter, every third letter follows in alphabetical progression to the end of the group, *i.e.* to *u*. This progression is completed by reverting to the third letter at the beginning of the group, and continues as before with every third letter. The second alphabetical progression begins after completion of the first and follows the same order of every third letter, to the end of the group.
(6) Letters of the first half of a sentence are interspersed with letters of the last half of the sentence, in reverse order.
(7) Each letter, reading forward, is combined with two letters, reading backward, thus forming a sentence.
(8) The letters of the alphabet are paired with and substituted by letters of the reversed alphabet.

Series 3. These experiments were planned to emphasize a fundamental similarity of the actional and elaborative functions. In both the situation holds something-to-be-done, which is complicated by the necessity for discovering a means-of-doing. There were eight variations of this experimental situation. The first four consisted in the visual presentation of a perceptual event which implied an orderly arrangement of a set of plain white cards (2 x 3 in.). *O* was instructed to watch the procedure and later to reproduce the original order of the cards. The cards carried identifying letters or numbers on the back (to aid *E* in checking the accuracy of *O*'s performance). In the first sets used, these markings were included in the occasion. *O* was instructed:

Each of these cards has a letter on the other side. In their present order, from top to bottom, the cards spell two words. I am going to change the order while you watch. You are then to discover the two wor s, not turning the cards over until you have formed a method of solution. Now watch.

As soon as *E* had completed the arrangement the set was handed to *O*. In later sets of the series the instruction was simply Watch! . . . Restore the cards to their original order.

The order of the cards before and after arrangement by *E* is given below, together with the method of arrangement.

(1) Number of cards = 17. Original order: cornelluniversity. Problem order: yirvnlerconluiest. Method of arrangement: *E* took the first card, placed the next under it and the next over it, continuing to place the cards alternately on the bottom and top of the new pack, until all the cards were rearranged. The order was thus one-under and one-over. The last card was on top. The first card was at the middle, with half of the remaining cards preceding it and half of the cards following it.

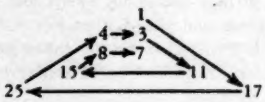
(2) Number of cards = 19. Original order: 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19. Problem order: 18 17 15 14 12 11 9 8 6 5 3 2 1 4 7 10 13 16 19. Method of arrangement: *E* took the first card, placed the next two cards over it and the next card under it. This alternation of two-over and one-under was maintained throughout. The last card was on the bottom of the new pack. The first card was preceded by two-thirds of the remaining cards and followed by one-third.

(3) Number of cards = 21. Original order: 21 20 19 18 17 16 15 14 13 12 11 10 9 8 7 6 5 4 3 2 1. Problem order: 17 16 15 14 10 9 8 5 4 2 1 3 6 7 11 12 13 18 19 20 21. Method of arrangement: *E* took the last card, placed the next to the last card over it and the second from the last under it. Continuing to take the cards from the bottom of the pack, *E* placed the next two cards on the top of the new pack and the next two cards on the bottom, the next three on the top, etc. The progression was, therefore, one-over, one-under, two-over, two-under, three-over, three-under, four-over, four-under. The first card was at the middle, with half of the remaining cards preceding it and half of the cards following it.

(4) Number of cards = 25. Original order (the cards were arranged on the table in the form shown below):

1
4 3 2
9 8 7 6 5
16 15 14 13 12 11 10
25 24 23 22 21 20 19 18 17

Problem order: 1 2 5 10 17 18 19 20 21 22 23 24 25 16 9 4 3 6 11 12 13 14 15 8 7. Method of arrangement: Beginning with card 1, *E* picked up the cards in the order indicated by the following diagram:



(5) In the fifth set of the series the instruction was merely, Watch! *E* arranged in a horizontal row on the table eleven cards bearing numbers in arithmetical progression. This was followed by three rows (each row being covered before the next was shown), in which the same progression was maintained but the members were uniformly increased by two. The four rows were then uncovered and shown together.

2	8	3	10	6	14	11	20	18	28	27
4	10	5	12	8	16	13	22	20	30	29
6	12	7	14	10	18	15	24	22	32	31
8	14	9	16	12	20	17	26	24	34	33

The arithmetical progression, typed on a card, was then presented.

$$+6\ -5\ +7\ -4\ +8\ -3\ +9\ -2\ +10\ -1$$

Examination of this progression will reveal that it contains within it a further progression in alternation, *i.e.* a *minus* progression from right to left: −1 −2 −3 −4 −5 and a *plus* progression from left to right: +6 +7 +8 +9 +10.

(6) In the sixth set the following progression was presented on a single sheet:

5 5 1 8 16 11 19 57 51 60

Following this was a sequence of 'clues' similar to those of *Series 2*. These were discontinued as soon as *O* had fully grasped the progression.

CLUES

(6) a. 5 5 1 8 16 11 19 57 51 60
b. 5 5 1 8 16 11 19 57 51 60
×1 ×2 ×3
c. 5 5 1 8 16 11 19 57 51 60
×1 ×2 ×3
−4
d. 5 5 1 8 16 11 19 57 51 60
×1 ×2 ×3
−4 −5 −6
e. 5 5 1 8 16 11 19 57 51 60
×1 ×2 ×3
−4 −5 −6
+7

(7, 8) Sets seven and eight reverted to the pack-arrangement of the earlier members of the series. In the 7th set were twelve cards. Taking the cards alternately from the top and bottom, *E* placed them successively in four piles so that the top card of the set was at the bottom of the first pile and the second card from the top at the bottom of the third pile. The total arrangement was, therefore, as follows:

Pile 1	Pile 2	Pile 3	Pile 4
5	8	6	7
3	10	4	9
1	12	2	11

Having placed the cards, *E* then picked up Pile 1 and placed it on Pile 2; then picked up the combined group and placed it on Pile 3, then picked up that group and placed it on Pile 4. *E* then picked up the entire pack and handed it to *O* to restore to the original order, *i.e.* from 1 to 12 consecutively. The sequence of the cards as given to *O* was, therefore,

5 3 1 8 10 12 6 4 2 7 9 11

The eighth set in the series followed the same procedure except that there were 24 cards which were placed in three piles.

I. Characterization of the Main Stages of Thinking

Our results suggest the following notion of thinking; as of a highly integrated performance involving a directive organization of various other psychological functions together with their products. It is further distinguished by its formulation of a problem which is advanced toward a solution. Examination of our data reveals three typical main stages in the total operation: (a) formulation of the thought-problem; (b) elaboration; (c) progress toward a solution (complete or partial solution or defeat).[18]

[18] The technical terms and abbreviations in the reports which follow conform (with a few additions) to the list established in Cornell studies in dynasomatic psychology, this JOURNAL, 51, 1938, 224.

act (A) = acting
em (E) = emoving
per (P) = perceiving
TS = task-setting
SI = self-instruction
OI = occasional instruction

(a) Formulation of the problem. Developing the thought-problem was found to be an essential phase of thinking. This was demonstrated both where *O* carried out the instructions to create a problem and where he failed. The importance of this primary activity of formulation has been almost entirely overlooked by previous investigators, who have assumed that the problem exists ready-made and has only to be somehow appropriated by the prospective thinker. This assumption has led to the omission of an essential stage of the performance.

Let us examine the character of a problem which is of the *thinking* kind. What are its distinguishing marks? And how does it develop? We find first the *problematic occasion, i.e.* a situation which is essentially obscure and unclear and which resists clarification. We say that it is puzzling or baffling. Its want of precise meaning is indicated by such ejaculations as 'What *can* it mean!' and 'It doesn't make sense.' Furthermore, it is qualified by a restless dissatisfaction (unpleasant in cast); dissatisfaction which is strikingly similar to the emotive predicament.[14] In both cases there is set up an active, vigorous search for a way out, *i.e.* for a means of resolution. Here are instances.

S (there must be something to be done with this; I wonder what it is) *cum* com ("This is awful!") > SI (make something; something must be done) . . . insp (what can this be!) *cum* com ("wish this were over!"); S_{α} (carried by general bodily posture forward; biting cheek in puzzlement); insp (I don't see what I can do; I'm stuck) . . . *La* 1-1

rem (R) = remembering
imag (Im) = imagining
insp (In) = inspecting
comp (C) = comprehending
T_{α} = elaborative thinking
S = searching
S_{p} = primitive searching
S_{α} = goal searching
som = somesthetic
kin = kinesthetic
vis = visual
aud = auditory
tac = tactual
com = comment (in words)
verb = verbal
fl = flash

FI = formal instruction
P_{f} = functional product
G_{e} = extra-organic government
G_{o} = organic government
G_{h} = historical government
[] encloses experimenter's or observer's remarks
() encloses functional products
. = complete break in function
: = closely related functions
cum = concurrent functions
> = successive functional phases

Functional moments included in compound performances are written adjectivally with a small initial letter placed as subscript to an abbreviated symbol for the main function. Thus, A_{p} = a perceptive moment in acting: E_{m} = a memorial moment in emoving. In this context, *imagining* and *inspecting* are distinguished by Im and In. Thus, In_{s} = inspecting qualified by search. Quotation marks enclose verbalized products.

[14] For a description of the emotive predicament see M. Bentley, *The New Field of Psychology*, 1934, 233-236.

S [to find a problem] *cum* organic strain and restlessness; insp (fruitless) > S [sighs heavily and looks at watch] . . . *Li* 1-4

S [for a problem] > fl (this is hopeless) > S [for a problem] > SI (damn it! there must be a problem here) . . . insp ("I can't do anything else with this"); S_{in} (I give up) > lapsing into idle per . . . *La* 1-5

This problematic occasion usually incites activity directed toward resolution. The incitement requires, however, that *O* see far enough into the situation to discover its puzzling nature, and our reports include many instances of failure to initiate performance necessary to the creation of a problem because of an inability to penetrate the occasion. Again, *O* may ignore the lack of clearness and occupy himself instead with whatever diversive substitutes may be at hand. Indeed, this resort to substitutive activity occurs at many points along the entire course of thinking. But when *O* grasps the situation, there results an active exploration which draws chiefly upon perceiving and inspecting moments. Note the following reports.

Per (whole field as haphazard) >SI (look for something to do) > insp (it's just a mess). per (this piece as having something glued to it) > insp (two pieces glued together) > OI (insp other similar pieces) > P_{abs} (material on part of several is the same) > TS (try to fit the pieces together) . . . insp (two different kinds of figured materials are attached to those composite pieces) > TS (do something with figured pieces) [dropped because of haphazard way in which figured materials were used] . . . *Ry* 1-4

Per (letters in special formation) > insp (a series of letters obviously spaced in word formation) > SI (what can it mean?) > fl (cryptogram); comp (cryptograms are solved on the basis of the natural frequency of letters) > OI > TS (determine frequency of letters) . . . insp (letters) *cum* com ("if these are words, they are very long; perhaps in a foreign language") . . . *La* 2-1

Substitutes for formulation. Thinking is especially difficult in this initiatory phase and a common device is to seek less demanding and more comfortable activities, thus avoiding bafflement and perplexity. Our common and everyday substitutes are prejudices, opinions, fixed beliefs, sheer guesses, and ready verbal tags. Since our experimental conditions did not favor these escapes, *O* was forced to invent some other means of resolving the difficulty at hand. At this invention, however, he was very apt.

The most frequent means was through imaginational construction. The essential distinction between this imaginational form of invention and that of real thinking is the lack in the former of any clarification of the baffling situation. The imaginational performance does not unravel; it embroiders. The situation remains obscure. The thinking performance, on the other hand, is directed toward clarification, and to some degree at least, attains it. We give below a few illustrative reports of problem-substitutes.

Per (pieces) > insp (good many round ones this time) > OI (I ought to be able to do something with that as a starting point); im_{vis} (a snowman) > in_{Im} (these pieces might be a snowman); SI (that could represent winter; flowers could represent summer) > TS (make a snowman) > act (arranging) . . . TS (now make something to represent spring) > insp (spring is usually when people are falling in love); im_{vis} (students walking hand in hand here on spring nights) > OI (how to do that with these pieces?) > insp (this piece might be the moon) > TS (build a bench and have a pair sitting on it) > act (arranging) > TS_{sub} (find a piece to be his arm around her) > act (arranging) > insp (completed figure); com ("well, I certainly like my man and woman; they look most loving") > insp (I hate to disturb my precious couple; I hope I don't have to do something else with them). [After FI: Create a different problem] S [for some problem which would not disturb the couple] . . . *Sb* 1-6

SI (make something) > random arrangement > OI (this arrangement looks like the profile of Sherlock Holmes) > insp (yes, there is his hat; there his scarf; and this piece is the turned-up collar of his coat) > OI > TS (arrange the other pieces to represent the profile of Watson; thus we will have a conference between Holmes and Watson) . . . *Li* 1-4

The persistence of the imaginational trend is particularly striking in this *O*, who reports in a later series:

TS (make a couple of jingles based on the words formed) > $insp_{Im}$ (lines of a jingle); SI (count feet in the line to make it rhyme) > act (writing and reading the lines); insp (jingle: Let the cod and the perch Rest in the sea With you and me. But let us not fall In the same old rut Like that foolish old crut, The Llama!)

It is clear from the above reports that this form of imaginational activity does not clarify the problematical situation.

Another diversive performance which was also ineffective in formulating the problem was that of idle, goalless psychological flitting, governed only by fortuitous instruction from the occasion. This roaming seems characteristic of *O* when he finds he must do something but does not care to exert himself. It also occurred in our *Os* whenever performance was blocked by the fruitlessness of prolonged search and inadequacy of the psychological products uncovered. In *Series 1* it took the form of random handling and arranging of the cloth pieces, accompanied by a running commentary; in *Series 2* of random selection and combination of letters, accompanied by attempts at verbalization. The following excerpts illustrate the latter form of this activity.

S (random selection and combination of letters) *cum* verbal pronunciation > insp (groups of jumbled letters) > S [merely gazing at the letters in the hope that they would become reorganized into words] . . . *Ry* 2-6

S [juggling the letters] *cum* comp, bare (there's nothing here that resembles the mechanics of previous problems) > comp (there must be something here, some method by which words can be brought out) > S [juggling letters without a plan] . . . *Li* 2-7

Finally let us consider those instances where the activities set into operation during this first stage yield a formulation of the problem. We include here not only a grasp of perplexing factors in the situation but also the positing of means of resolving them. Any of the psychological functions

may be here operative, either singly or in integrated forms. Furthermore, the products of these functions are so referred to the problematic occasion as that they are either discarded as useless or retained as significant to the problem and possibly to its solution. In the following examples we find perceiving and inspecting constantly employed to turn out useable material.

Insp (here is a series of letters with several possibilities: 1, letter sequence; 2, sentence sequence; 3, cryptogram; 4, isolated words hidden; 5, nonsense grouping; 6, some other grouping) > TS (determine whether any of these is correct) . . . *La* 2-1

Per (period at the end of the three groups) . . . TS (try to make a sentence) . . . *Sb* 2-1

Per (jumbled letters) > TS (unscramble) . . . per (two gaps between the groups) > TS (make the letters into three words) > TS (look for vowels) . . . *Ry* 2-1

TS (identify as many of the materials as possible) > insp (materials) > insp (this piece seems to be made of two layers; paper pasted on the back of cloth) > TS (inspect it carefully to determine whether the paper backing is there simply as a matter of chance) . . . per (this piece is more of the same material) > TS (look for other like pieces) . . . *Ry* 1-2

Thus when performance is clearly directed toward an anticipated solution we may say that the problem has been fully set and developed and that the initiatory stage in thinking is—at least temporarily—complete. As we have pointed out, the characteristics of this stage include an initial bafflement induced by an occasion which is really problematical to *O;* a search for means of discerning and segregating the perplexing factors, or an escape by some substitutive device; the positing of various methods of attack; and, finally, the setting of the operation in the direction of an anticipated solution.

(b) Elaboration. It must not be supposed that performance is always as clearly marked off into well-defined stages as may be suggested by our general plan. Many variant arrangements of moments and stages occur. Nevertheless, activity does take a new functional turn as soon as *O* has created a fresh problem and has thrown it into commission for solution. Then comes elaborating, sometimes tentative, halting and vague and sometimes direct, progressive and effective. In both cases methods of solution are proposed, tried out, amended, rejected, and followed up.

Within this elaborative stage are distinguishable three important phases, *i.e. (i)* the accumulation of material, *(ii)* valuation and selection, and *(iii)* fusion and synthesis of selected products.

(i) The accumulation of material. The first step in elaboration is assembling 'food for thought.' Any or all psychological functions may be in-

volved. The apprehensive functions and comprehending, however, are most active. We cite a few excerpts exemplifying the extensive collection of gross materials by perceiving, imagining, remembering, and comprehending. It will be seen that imagining and remembering serve to amplify the products of perceiving by extending the occasion into the past and future; while comprehending enriches and refashions the apprehensive products by yielding new significations.

Per (third card placed in the opposite direction from the second) > im_{ant} (the plan will be alternation) > per (cards placed in alternate arrangement as anticipated) . . . *Ry* 3-1

Per (abcde) > OI (what is the significance of that?) > TS (discover the relation between this and the top line) . . . *Li* 2-1

Im_{ant} (something that could be put together) > im_{ant} (some pieces will have paper glued on the back) > TS (look for pieces with paper glued on the back) > per (this one is stiff); com ("that's one") > TS (try to fit pieces together into a flag as before) . . . *Ry* 1-6

Rem (the two top cards were taken from the bottom of the original pack) . . . TS (determine how to reverse it) . . . *Ry* 3-2

Per (something different: a z) > SI (what does it mean?) > comp (alphabet); rem (previously there were alphabetical progressions and I tossed them out) > fl (this is right) > TS (find the progression and eliminate it) > insp (the progression is not here) > SI (now what to do?) . . . SI (what does a z mean?) > SI (why the space between a and z?) > comp (substitution: a is to be substituted for z, and z for a; b for y, and y for b; c for x, and x for c, etc.) . . . *La* 2-7

Per (figure) > insp (two arrows with connecting line) > OI (what does it mean?) > comp (it means start somewhere, go as far as you can, then turn back to a new beginning place and proceed until again stopped) > S (relation of this to the problem) . . . *La* 2-5

Per (placement of third card in the opposite direction from the second) > comp (the plan is alternation) > per (placement of cards in alternation); TS (how can I sort them out again?) > insp (first card will be in the middle) > S [order? was the second card placed above or below the middle card?] > TS (count the cards to find the middle one, then see which of the two before and after it best combine with it to form a word) > Im_{vis} (cards arranged on the table) > comp (I can work back, alternately taking cards from the top and bottom) *cum* OI (watch to see whether the last card is on the top or bottom) [as last card is placed] per (last card is on top) > OI > TS (start with the top card; then take the cards alternately from the top and bottom) > act (arranging) . . . *Ry* 3-1

The most important functional tool in elaboration, however, is inspecting. The critical scrutiny which is inherent in this function is essential to thinking, especially during the elaborative stage. It is by means of inspecting that the procedures suggested in the formulation of the problem are tried and tested.

SI (maybe there is an alphabetical sequence in reverse) > TS (try every third letter beginning at the end) > insp (no reverse sequence) . . . TS (try every third letter combined with the two letters following it) > insp (no such sequence; only three words, probably there by chance) . . . *La* 2-1

Per (something different here) > insp (two arrows pointing the same way) > S (what can that mean?) > fl (something backwards) > fl (of course, sentence backwards) > SI (why two arrows?) > insp (*the* at end of the sentence, letters separated by single letters) > TS (proceed by eliminating alternate letters) > insp (the alphabet) *cum* fl (this is right) . . . *La* 2-5

TS (take alternate letters beginning with the period) > insp (four vowels: unsatisfactory to form a word) . . . *Ry* 2-6

Per (word *task*) > TS (determine the pattern) > insp (t--a-s-----k) > comp (no regularity there; it must be wrong) . . . *Li* 2-6

Insp ("over under over") *cum* fl (same as before) > per (fourth card being placed in the first pile; not what I had anticipated) > com ("just three piles this time") [insp had lapsed because I thought I had the plan; with per of anticipated placement, there was a brief panic with self-reproach, "I've got it all mixed up now; I should have watched"] > SI (watch carefully now) > insp ("over under over, etc.") *cum* fl (relief); comp (It's really not different with three piles; the first pile contains cards which come alternately from the top and bottom; the plan is still alternation) . . . *Sh* 3-8

These reports reveal two extremely important aspects of thinking: first, the variety and the wealth of psychological material which is collected during elaboration and, second, the diverse and fruitful modifications of functional trend by way of antecedent performance, a type of progressive modification which is of the very essence of thinking in all its phases and stages from task-setting to final solution. It stamps thinking as of the same functional order as acting, where also the operation is continuous, forward-tending and cumulative.

(ii) Valuation and selection. The many products which accumulate during elaboration are often extremely heterogeneous and unrelated. By critical appraisal (well illustrated in the above reports), this raw material is refined and sifted to a more orderly mass of selected products, which bear more directly upon the problematical occasion. This valuing-and-selecting is usually carried on, in our experiments, through inspecting and comprehending. The obscurities of the occasion are thereby partly clarified and performance moves toward solution. Our facts indicate that the appraisal and the subsequent selection or rejection of products, involving the ability to distinguish serviceable and relevant from non-serviceable and irrelevant materials, is a basic factor in sound thinking. It is therefore obvious that the inculcation of this form of comprehending and of inspecting is of prime value in acquiring the art of thinking. This aspect of the really elaborative form of operation also serves to distinguish it clearly from all those behavioristic forms of 'problem-solving' which exploit trial-and-error, associative learning and conditioning.

We should note, however, that appraisal does not lead immediately to solution. There are many reversals, revivals, rehearsals and reconstructions

in the course of thinking. Values are frequently misapplied and performance often misguided. There is an almost constant shifting in the occasion as newly-fashioned products throw into relief now one aspect of the problem and then another. It is easy to be lost among these changing forms and estimations, and correspondingly difficult to detect the essential aspect of the occasion and the valuable means of reaching a solution.

(iii) Fusion of selected products of elaboration. Although the three moments in elaboration are not always to be found as temporally distinct and of a fixed order, the fusional and integrational phase is logically last, for here occurs the final synthesis of selected materials whose product is the solution, although preliminary fusings and integratings may serve the thinker at any stage. The functional processes of integration are very subtle and often highly condensed. In fact the sturdy and ancient belief in what men like to regard as 'intuition' and 'insight' suggests the sudden discovery of great truths as a flash without any intermediating process in the mind of the thinker. While this belief rests almost wholly upon a legend of the 'divinity in man' and is wanting in experimental evidence, our own scanty results do not go very much beyond a definite and empirical statement of the problem of functional products with or without underlying and corresponding functional procedures of a psychological kind.[15] Until the problem has been carried through under a sound laboratory procedure very much more exacting than it has ever been, the existence, the nature, and the range of activity of an alleged 'intuitional' or 'insightful' faculty of thought should be regarded as a convenient, comforting and flattering hypothesis. Since the entire psychological development of man, especially from adolescence on, abounds in condensation, ellipsis, and functional short-cutting, nothing stands in principle against the production of large and significant issues from obscure and unobserved facts. More definite conclusions await a great increase in knowledge of the bodily conditions of elaborative thinking and of the psychological procedures which are involved. Until now the neurologist and the behaviorist have added very little in either direction.

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[15] As regards the actuality of 'insight' (as a psychological category), main references and an estimate of the evidence may be derived from M. E. Bulbrook, An experimental inquiry into the existence and nature of insight, this JOURNAL, 44, 1932, 409-453. Another way of approaching the problem is through the experimental study of the *flash* or *Bewusstseinslage*. See M. Bentley, *The New Field of Psychology*, 1934, 291, 303-306, 405-410.

The case for intuition in the sense of "sudden illumination" is cogently presented by H. Poincaré, *Science et méthode*, 1908, 43-63. The appeal is to various unconscious agents and to an esthetic sensibility.

The following excerpts, chiefly taken from the final stage of elaboration in our experiments, will at least show that, under our conditions, wherein *O* created and worked through his own problems of thinking, the functional phases of collecting and reassorting his materials in the elaborative stage led up to, and issued in, integrative procedures which passed beyond these intermediate products, by something like an inferential process, and suggested what was to the observer a new solution.

Per (a = z); comp, flash-like (a equals z; therefore substitute a for every z) > insp (if a equals z, then b equals y, etc.) > TS (translate the letters in that way) . . . *Li* 2-7

TS (identify arrangement) > insp ("first card out, second top, third bottom, etc.") > insp ("if there are an uneven number of cards, the middle card will be the first letter, the card above that will be the second letter, the card below, the third, above the fourth, below the fifth, etc.") > TS (count the cards to see if they are an uneven number) [counting: 17] insp ("uneven; *therefore* the ninth will be the middle card and the first letter") . . . *La* 3-1

Rem (last two cards on top were identified as cards from the bottom of the original pack) > comp (since I started with the bottom cards, they are now in reverse order) > TS (how to reverse?) > im_{vis} (pack turned over) *cum* com ("I can't turn them over") > R_{gen}, vague (shuffling playing cards) > OI > TS (take them off the top in that way) . . . *Ry* 3-2

Insp ("it's down, around, reverse") *cum* comp (the scheme is a wavy line) > insp, relaxed (*E* is doing it as I anticipated) . . . rem (the top card is the last card) > comp ("*therefore* the first card I place will be the right-hand card of the second row") . . . *La* 3-5

Looking back upon the elaborative phase at large, we may say that the single word which seems best to express the entire procedure is 'manipulation,' *i.e.* herein the organism manipulates a great variety of psychological products. A fairly small fraction of these products are made under the guidance of the environment, of what the behaviorist calls *stimulus*. These are perceptive products. The larger fraction are turned out, in relative independence of the environment, from other resources of the organism under the guidance of a general *task* and of the specific thought-problem which the organism has created. The products once given, all along the course of elaboration, manipulation consists in extending, revising, condensing, and crystallizing, in preparation for a new product which is to emerge (where thinking is wholly successful) as the end-sought-under-the-problem.

In summary, the distinguishing contribution of the period of elaboration is (1) a collection of varied psychological materials; (2) an ordering of these into the relevant and the irrevelant materials through a critical appraisal based on their 'worth'; (3) a further scrutiny of 'accepted' products; (4) integration into a unity where products have been not only

related to one another but to a general topic which is the anticipated solution.

(c) Solution or defeat. The final stage may emerge in solution, partial solution, or defeat. The following reports are examples:

. . . Com ("there's nothing more to this") [after clue x1-x2-x3 is presented] > per (startle) *cum* fl (this isn't over) > insp (5 × 1 is 5; 8 × 2 is 16; 19 × 3 is 57) > com ("now what?") > S [to continue this pattern] > insp (various patterns) . . . [with next clue] > S [what more?] > fl (if you drop the signs the whole thing is a progressive series from 1 to 9) > insp (within that progression there is also a multiplication progression of 1-2-3, a minus progression of 4-5-6, and a plus progression of 7-8-9) . . . *Li* 3-6

Per (same group as before) > rem (card previously shown) *cum* S [will those differences fit in here?] > rem (only plus and minus signs on card) *cum* comp, sudden (the card gave the composite plan of the series) > per (28 : plus six) [checked several more differences] > com ("that's what it is; queer that I didn't recognize it") . . . *Ry* 3-5

Per (*strive:* peculiar arrangement of the letters) > TS (determine the pattern of arrangement) > insp (pattern) > TS (find the next word by using this pattern); com ("I can't quite get the principle here") > SI (try to formulate the principle by a diagram) > insp (various diagrams) . . . S ("what can possibly be the principle here?") > insp (possible progressions are 1-2-1, 2-1-2, 3-1-3) > insp (those combinations won't work); com ("no, that's wrong") *cum* posture indicating defeat . . . *La* 2-5

Per (individual numbers) *cum* subvocal pronunciation of differences between pairs > comp (differences didn't follow any plan) > TS (look for pattern in alternate numbers) > insp (productless) > TS (try every third number to see if they form a pattern) > insp (5 8 19; 51 as not fitting in anywhere) > per (large numbers at the end; series going up very rapidly) *cum* (the plan is probably something like adding 8, subtracting 14, adding 25) > SI (there must be something easier than that) > per, roving (16 11 19, jump to 57) *cum* com ("simply can't be any plan in this thing") . . . *Ry* 3-6

Per (4 5) *cum* com ("maybe it is a progression after all") > per (12) *cum* fl (it is going to be a progression: 12 fits in: it's 4 10 5 12, one group of alternates progressing by one, the other by 2) > per (next numbers as going down) *cum* fl (it is not right after all: doesn't fit that plan) > insp (last card is 29; next to the last is 30) > OI (maybe they go backwards) > insp *cum* fl (dismissal of that) . . . *Sh* 3-5

It is not to be assumed that the products of thinking are confined to the final stage of solution. Numerous products result during the entire course of performance: discoveries, decisions, appraisals, acceptances, rejections, etc. These do not invariably lead to solution, but they are nevertheless thought-products. In the following excerpts, for example, we find a number of instances of such thought-products without solution of the primary problem.

Per, vis. roving (groups of letters) > TS (try some conventional pattern, *e.g.* take first letter of first group, second letter of second group, third of third group) > insp (*gie*) *cum* comp (probably wrong; *gie* not part of a word) > per (*h;* rapid pronunciation *gieh:* meaningless group of letters) . . . com ("simply can't make any words out of this because there aren't enough vowels") > comp (series have been

getting more and more difficult) > comp ("this is probably insoluble"). [After being instructed to continue] TS (since I can't make words, look for some pattern of regularity) . . . insp > S ("this is really useless because it's impossible to make words and I can't find any regularity of arrangement") . . . *Ry* 2-7

Insp$_r$ (the others formed sentences) > OI (so will this: the word *the* or *this* is probably here) > insp (*t* in second group; *he* in third) > insp ("looks like one-forward-two-backward progression again") > TS (try that with the beginning) > insp (*gmr:* no word there) > insp (*sh* at beginning: could be a word) > OI (continue alternating) > insp (*shiz:* no word) > insp (there seem to be many consonants but a dearth of vowels: that's strange) > insp (in third group, *helm* is in reverse order by alternation) > OI (try that plan) > insp (*mrfrhf:* that's no good) > SI (maybe I should start someplace in the middle) > insp ("but there's no way of determining where: I can't make any sense of this: so few vowels") > insp (maybe there's some sort of substitution here) > rem (I did that once before and it didn't work; anyhow it's out of the running of things so far) > SI (there must be some progression here) > com ("if I can't get it, there'll be a clue; I'm stuck") . . . *La* 2-7

Insp (first group) > TS (try various combinations to get the word) > com ("it won't make a word because there are no vowels") > fl, fleeting (maybe I should substitute a for z, etc.) [dismissed] > insp (every other letter, backwards, in the third group gives *helm*) > insp (that doesn't work with the other group: dismissal) . . . com ("I don't believe it can be anything as it is because two groups have no vowels") > TS (put the groups together) > TS (trying various combinations) > insp (dismissal: emotional toning of unpleasantness) . . . *Sh* 2-7

TS (find the hidden words) > insp (*dd ee*) > TS (see if there are more paired letters) > insp (no). TS (find some pattern) > insp (dd-ee-t-jump-t: rejected as not representing a pattern) > In$_s$ (fruitless) > insp (various patterns tried and rejected: "this is not the pattern") > SI (maybe the *es* have something to do with it: see how frequently they occur: see if there is any pattern in their arrangement) > insp (*e,* gap 5; *e,* gap 3; *e,* gap 3; *e,* gap 5; *e,* gap 4: gave up) *cum* fl (this isn't getting anywhere) . . . *Li* 2-5

Per (numbers) > TS (see if there is some arrangement like the last one) > insp (differences) [dismissal of that since there were no consecutive differences, like 0 1 2 3] > SI (maybe something is multiplied each time) > insp (dismissal) > SI (something divided?) > insp (dismissed) . . . *Sh* 3-6

In our discussion of halt or lapse of function, differences in *rate* or *tempo* of performance have been mentioned, differences due to the failure of products to provide for a functional advance toward the anticipated goal. In sharp contrast to this slowing down, there is also a quickening or sudden revival of performance due to the emergence of newly-significant products. This quickening may occur so suddenly that *O* is scarcely aware of the means of facilitation. The course of thinking thus progresses unevenly; in one phase it may advance 'by leaps and bounds,' in the next phase it may be completely stopped. Again, it may proceed at a steady but slow pace, or with alternations of advance and retrogression. Rarely, if ever, under our conditions, is thinking consistent and unimpeded in its advance toward solution.

II. The Government and Guidance of Thinking

Like acting, thinking is a directed operation, governed and directed from stage to stage by certain guiding devices both within and without the organism. Of all the psychological functions, the guidance of elaborative thinking is most complex and most delicate. In fact, so delicate are the means for keeping it homeostatically and effectively on its course toward effective conclusions that few men have mastered the art of hard thinking, most substituting for its arduous management—as we have observed—such easier devices as blundering trial and recovery, opinions, prejudices, imitative acting and believing, and verbal formulas. First among the means of direction and control of thinking stands *the task*.

The importance of the task is clearly demonstrated in our reports (where it constantly recurs), *O* sometimes discovering as many as a hundred tasks in a single observational session. These tasks exert an influence in two directions, the one *positive*, carrying the operation in a direction already established, and the other *negative* or away from the established course. Of positive tasks there are many varieties, depending both upon the demands of the problem and upon the specificity of a particular procedure. Tasks are also to be distinguished by the anticipative forecast of the thought-product presently to issue.[16]

The contrast between task-setting under a problem and task-setting independently of a problem is brought out in the two reports which follow. In the first, the tasks are all directed toward a solution, while, in the second, task-setting proceeds without reference to solution.

[Problem: find the sentence hidden here, by eliminating letters] TS (try eliminating alternate letters) . . . TS (try eliminating every third letter) . . . TS (try every third letter backwards) . . . TS (try alternate letters beginning with the period) . . . TS (try alternate letters, beginning with the period, going backwards) . . . *Sb* 2-6

Per (many pieces: like a quilt) > TS (do something with them) > insp (some pieces are cut in definite patterns) > TS (make a design of them) > insp (these are all alike) > TS (make a border of them) > insp (some fit together) > TS (collect them and see what can be made of them) . . . TS (fit them together) . . . TS (fix these leaves into a design of a growing flower with the long leaves at the bottom) > act (arranging) [during arrangement, several 'crucial,' *i.e.* pennant pieces are turned over revealing the paper backing, but *O* looks at them inattentively, carefully returns them to their original position, and puts them aside] . . . *Sb* 1-1

An essential characteristic of task-setting is its derivation from previous functional products. The task may emerge from the immediate or from a

[16] It is well here to clarify the distinction between the problem and the task. In the task, emphasis is on a way-of-doing, a kind of instruction which may be employed in any psychological operation. The problem, on the other hand, always predicts a possible solution and includes at least tentative instructions for attaining it.

past occasion. In our experiments, it most frequently came out of a present occasion. Perceiving and inspecting are especially valuable in yielding materials for task-setting, as is evident in the following reports.

Per (black piece) > TS (look for other black pieces) > P_{abs} (black pieces as a distinctive group); com ("they're in a group by themselves and have nothing to do with the others") . . . TS (inspect them further) > insp (paper backing) > OI (turn over) > act *cum* per (turning over: disclosing letters S and T) > OI (another banner to be put together) > TS (find the rest of the pieces in order to complete it) . . . *Ry* 1-3

Per (one piece backed with paper) > OI (is there any significance to that?) > TS (leave that until later; proceed with grouping the circles) [inadvertently turned over a backed piece] > insp (this must be the right side) > OI (there must be some significance to the backing) > In_{im} [interrupted by] SI (continue with grouping the circles) . . . *La* 1-6

Insp (some are backed with paper) > OI (what to do with them?) > TS (use to construct something different) [undirected movement arranging pieces at random] > fl (maybe these should make something; maybe they should all make something) > insp ("damned if I know what it would be; they're so different") . . . *Li* 1-1

It is interesting to note in those cases (which were paralleled by all *O*s who did not discover the significance of the 'backed' pieces) that, although perceiving or inspecting is directed to a part of the occasion (which might be expected to effect a turn in the direction of performance) the products turned out are not sufficiently meaningful to be effective.

This is a significant laboratory parallel to the situation in daily life where a clue to the solution of a problem is disregarded because of the inability to view it impartially. There is so great a 'set' toward a particular signification that the meaning which would be obtained through an unbiased envisagement is inhibited. For example, in a report cited earlier (*Sh* 1-1), *O* was so intent upon the 'artistic' placement of leaves that the backed (pennant) pieces were regarded merely as not-leaves and therefore cast aside.

When there are indications that the performance is not making for solution, *O* must be sufficiently alert to observe it and not so determined by a present functional inclination that he does not retrench. It is evident from our research that inflexibility of this sort is the source of much inadequate and wasteful thinking. It leads to an uneconomical use (or a total neglect) of resources at hand. Too great a fondness for one's performance is also a handicap in effective thinking because it prevents a critical appraisal of products and outcomes.

Apropos inflexibility, we have discovered a striking consistency of performance in each *O*. For example, one *O* inclines toward imaginational creation and will view any situation as potentially creative in that sense; another attacks it as he would a mathematical problem and is engrossed with plans for adopting suitable procedures. Still another constantly vacil-

lates from one view to another, quickly proposing and as quickly abandoning suggested formulations and processes. He is commanded by, but does not command, the situation. In general, we have noted a certain skill of following through sufficiently but not too far; a certain discipline of sticking long enough but not too long, which develops the degree of flexibility sufficient to meet the requirements of the thought-problem.

As we have pointed out, our experiments included the presentation of a sequence of clues which would redirect performance toward solution when a digressive course was too persistently maintained. The following reports show how task-setting evolved from the comprehended clues.

Per (arrows pointing this way) *cum* comp (letters should be read backwards) > TS (read the letters backward to get the word) > insp (*that*) *cum* S (pattern in *that*) > insp (arrangement: t, skip a, return to a, then skip four places) . . . *Li* 2-5

Per (a z) > C_{bare} (a z means something that I am to use) > S (what does it mean?) > per (there are no *as*) > comp (perhaps it means the first and last) > TS (take first and last letters of each group to see if they make words) > insp (no, nothing there) *cum* fl (that's wrong). $insp_{bare}$ (manipulation of letters: rejection because the combinations do not form words) . . . *Li* 2-7

Per (something different: a z) > S (what does it mean?) > com ("a to z") *cum* C_{bare} (alphabet); rem (previously there were alphabets and I tossed them out) > fl (this is right) > TS (find the progression and eliminate it) . . . *La* 2-7

[After presentation of clue sheet] Comp (solution of the problem is by substitution, a for z, etc.) *cum* self-reproach ("why didn't I do it, I thought of it but was too lazy! Why did I have to wait until I was told!") . . . *Sh* 2-7

Devices for maintaining the task. The devices by which *O* keeps the task clearly before him are varied. The most frequent is linguistic, *i.e.* he attempts to formulate in words what is to be done. The words serve as a shorthand reminder and taskmaster to keep the task alive. Such linguistic devices are numerous in our reports. We cite a few examples.

Insp (arrangement) *cum* Im_{ant} (cards-about-to-be-picked-up) *cum* S (to phrase verbally *E's* procedure) > com ("the arrangement is circular") > insp continued (the circular order is maintained) > SI (remember the order) . . . *Li* 3-4

Insp *cum* act ("top, bottom; top, bottom; top, bottom; top, bottom.") [placing each card in the appropriate piles, the word 'top' or 'bottom' served as OI for the placement of the next card.] . . . *La* 3-7

Again, the task may be embodied in a non-verbal form for guidance. An interesting example is the *emphasis-posture,* which serves to extract, through the task, appropriate products. One service of the emphasis-posture is that it ignores unuseable parts of the occasion. For example, in the excerpt given below, this device enables *O* to disregard the irrelevant letters.

TS (try to disregard all letters which do not form a word); rem (*Gestalt* theories of perception) > insp [by moving eyes over the whole group] (*alternate:* standing out from the other letters). insp continued [by the same method] (*letters are irrelevant*) *cum* fl (this is how it can be done) . . . *Li* 2-2

Rivalry and guidance. Another similarity to acting appears in the 'conflicting' or 'rivalrous' tasks of thinking, one of which becomes predominant and thrusts out another. At times rivalry attaches directly to inconsistent or conflicting products. A perceived object, *e.g.* may so predominate in a problematical setting that it leads to the abandonment of other similar objects. This rise-and-fall of rival factors of guidance is also similar to the attempts at emotive resolution. Much remains to be done to clarify both the similarity and the difference of elaborative thinking and the rivalrous and predicamentive aspects of emoving. In fact, the whole problem of initiation and guidance in psychological activity has been veiled in a complacent acceptance of such non-explanatory blanket terms as motive, purpose, drive, choice, and the like. It is abundantly clear, however, that governing factors are of great and especial significance in thinking, a performance which may, under adequate government, go steadily forward along a more or less fixed course toward an appropriate final term.

Summary and Conclusions

The study was prompted by the need for a descriptive account, at once methodical and controlled, of the nature of thinking. Our primary aim has been to determine and to depict all the phases of this characteristic operation and to discover the means employed by man for its initiation and guidance. The experiments provided the observer with a variety of occasions designed to provoke thinking and to carry the elaborative processes through to a conclusion. Only laboratory psychologists who were expert in observation and report were used as observers.

We derive from the results the following facts:

(1) Thinking is a functionally distinct psychological performance in which any combination of the simpler psychological functions may be operative.

(2) Three distinct phases in the total course of thinking are formulation of the problem, elaboration, and solution (or defeat).

(3) The period of formulation of the problem is marked by bafflement and search for the significance of an obscure and puzzling situation. When formulation has been effected, a general procedure for reaching a solution is developed.

(4) Elaboration involves accumulation of psychological products, valuation, critical selection and integration.

(5) Solution of the problem derives from elaboration. The final stage in

thinking may, however, yield only partial solutions or it may end in defeat.

(6) The entire course is directed toward the solution of a problem.

(7) Thinking progresses by the sustained guidance of frequently revised tasks.

(8) Tasks are constantly refashioned by assimilation of previous functional products.

(9) Thinking is difficult and is frequently avoided by resort to less demanding activity.

(10) Numerous products (such as discoveries, decisions, doubts, deductions) issue throughout the course of thinking.

(11) The final stage in thinking may not yield a solution of the primary problem.

(12) Thinking is tentatively described as a distinctive, highly integrated performance involving a directive organization of various other psychological functions together with their products.

A FUNCTIONAL AND DYNASOMATIC STUDY OF EMOVING

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No adequate descriptive account of emoving has ever been published. In spite of the fact that 'the emotions' fill one of the longest chapters in psychology and that emotion is now a favorite subject of research and of speculation, a coherent and first-hand description of what the living organism is actually doing when emoved is still wanting.

The general subject has been approached from four main directions; physiological, pathological, expressive or indicatory, and theoretical.

The *physiological* approach has led, according to a recent study of feeling and emotion, to nine topics or methods of research, all of them aiming at a quantitative account of bodily processes incited by 'emotional stimuli.'[1] The general tendency here has been to resolve emotion into bodily process and state; although some physiological writers have assumed (without defining) their readers' awareness that emotions are conscious things with bodily causes, accompaniments, and modes of 'expression.' The logical slip in 'emotional stimuli' is that they are not energetic agents exciting neural organs but homespun 'situations' from the physiologist's experience which have made *him* 'fall into an emotion.' As turbid a use of the causal concept as one is likely to find in science!

The *pathological* approach has yielded little of value for a descriptive account. The clinician usually borrows his notions of the subject either from the physiologist or (more often) from speculative theories which make the emotions dynamic agents in the causation of mental disease and disorder. Since the clinician's observations are, as a rule, based upon the translation of gross behavior into terms of 'expression' (a set of indicators found by experiment to be almost wholly unreliable, so far as actual emoving is concerned), no sound psychology is likely to receive benefit from the pathological side. Both because of his training and because he is immediately responsible for the patient, the pathologist depends heavily upon the psychological vernacular of the sick-room and easily falls into the folk-belief in mental causes and spiritual agencies. This belief reflects a doctrine of emotion not credited in the laboratories of the careful experimentalist.

A conjunction of the physiological and the pathological approaches has led to the current doctrine of cortical, subcortical and autonomic centers for emotion. The theory would promise more if it were based upon a reliable account of emoving itself, or else were content to be a plain account of bodily areas and processes devoid of 'emotive' pretensions.[2] Again, it is as impossible to know emoving and emotions

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[1] C. A. Ruckmick, *The Psychology of Feeling and Emotion*, 1936, 283-285.

[2] *Cf.* M. Bentley, The psychologist's uses of neurology, this JOURNAL, 49, 1937, 233-264.

without a first-hand command of other psychological functions (especially the other problem-positing functions of thinking and deliberative acting) as to know the reptiles without a cross-reference to the amphibia and the birds.

The *expressive* side of emotive functioning rests upon the observation, as impressive as it is common, that face, mien, posture, voice and movement offer rough but useful indicators of human intent, feeling and conviction. Men communicate by dexterous use of these bodily signs. Darwin's fascinating study of 'the expression of the emotions in man and animals,' as well as the biological theories which have derived from it, have tended to narrow the indicators to those of the face and the indications to the emotive variety. Where they have not known the forms and varieties of emoving itself, physiologists and psychologists have made this rough and ambiguous theory of signs into a substitute for the processes signified. Thus the growl of the dog lashed to the operating table has been read as 'anger' and the trembling of the mouse as 'fear.' That is as if the discovery of red pigments in a wilderness were to be taken as a sure indication that 'red men' were in the region. In the second place, the term 'expression' suggests that it is of the nature of emoving *to express itself*, an assumption quite gratuitous and one which wholly confuses the socialized onlooker with the emoved organism. Had it not been for this confusion, it is doubtful whether we should have had three decades of footless discussion upon the argument whether 'we cry because we are sorry' or 'are sorry because we cry.'[3]

Theories of emotion have been established upon the generalizations of biology. The presupposition that bodily organs and actions are useful was long ago extended to this subject. Emotions have accordingly been found to be designed for defense, warning, display, augmentation of energy, and so on. The more recent theories have dwelt upon the emotive response and its relation to special devices in the brain, endocrines, and elsewhere. Nothing like a classification of bodily resources for the various classes of emotion is to be expected until the emotive modes have been themselves differentially described and definitely related to the psychological functions of perceiving, imagining, comprehending, acting and thinking. Although the psychological theories of Lehmann, Wundt, and some others, which have sought to relate the specificities of emoving to circulatory, respiratory, and muscular processes have moved in the right direction, the facts taken into account, both psychological and physiological, have been too limited for an adequate construction.

We have urged a careful description of the processes of emoving as the central need at present. All four main approaches seem to us to have been blocked at many points by the fact that expositor, theorist and researcher have alike been embarrassed by the want of specific and reliable knowledge upon the nature, the temper, the variety, and the time-course of emoving, as well as of the relation of the emotive operations of the organism to the other psychological functions. It is as if the physiology of organic respira-

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[3] *Cf. Feelings and Emotions: the Wittenberg Symposium*, 1928, 17-23; M. Bentley, *The New Field of Psychology*, 1934, 235, 243; A. M. Feleky, The expression of the emotions, *Psychol. Rev.*, 21, 1914, 33-41.

tion were to be resolved without knowledge of the course of the respiratory acts and of the chemical participation therein of oxygen and carbonic acid, or the racial history of the hominids without a comparative anatomy of primates.

Emotive description shares with the experimental accounts of thinking very great and unusual difficulties of technique and of training. We cannot hope to make more than a bare beginning, a beginning which should, however, be enriched by parallel studies, especially studies in this series of researches, which bear upon comprehending, perceiving, remembering, searching, acting and thinking.[4]

Our experiments have been directed toward conditions simple enough to admit of report and record and complex enough to induce real emoving of definable sorts. The high similarity of emoving to certain forms of the better-known function of acting has suggested that we provoke occasions where the relations to acting were first remote and then intimate. At the end it appears that we might well have reversed the order and so had the support, from the beginning, of the relatively full knowledge which psychologists have acquired over several decades of the variant courses of the several sub-modes—simple, selective, abbreviated, elaborative, etc.—of the actional forms of executive functioning.

So widespread is the claim which emoving makes upon the organism that men have always found it difficult to guarantee a fair sample of this operation and, at the same time, to reserve enough resource to note and to record. The experimenter has had to resort to every kind of device and control. The most popular (possibly because it is the most spectacular) device has been to subjugate and then to bully the subject. Darkness, isolation, strapping, effrontery, paining and terrifying have all been tried. Like a major earthquake, an overloaded battery, or the judicial electric chair, these means have produced results. The kymographic pointer has left the smoked paper in a dizzy swing; sweat has oozed; the pneumograph has snapped; the galvanic needle has swung beyond its range; and profanity has eased the tantrum of a perspiring sufferer. Plenty of results; but what of them? Well; the advocate of expression and of response has found enough of both and he has been at pains to devise cunning experimental means of registration; but what his records 'express' or what the organism has 'responded to' he cannot say further than to refer to an 'emotional stimulus' or to declare that he has been working with one of the

[4] For these studies see this JOURNAL, 49, 1937, 343-418; 51, 1938, 253-356; 52, 1939, 161-185. The present writers had the advantage of observing in Dr. Chrisof's concurrent study of thinking.

textbook forms of 'affect,' usually anger, pain or fear, a trinity of unknown gods for twenty-five years esteemed alike by behaviorist, physiologist, neurologist and clinician.

In our own preliminary and exploring trials we inadvertently fell far enough into the heroic method of capture and torture to discern the extent to which results may be thereby invalidated and also to warn us against its more obscure and subtle forms. In these exploratory trials a predicament was induced by a slight, variable, but presumably innocuous, electric shock delivered on *O*'s forearm. First observations made by the director of the laboratory were carried through without incident; but it appeared after some time that the wide variability of the shock and of the *Os*' limens for pain permitted extremely unpleasant moments which were laid resentfully but without comment against *E*. Unluckily the determination to carry on without complaint submerged the resentment and blocked it from the reports. Only an outside inquiry brought out the fact that an experimenter's slip had delivered into our hands a beautiful emotion of increasing violence, but an emotion which wholly escaped the record. Although these early trials were thus damaged by the enemy of work of this kind upon emotion, they rewarded us by contributing considerable new information upon the rise and character of predicament and the nature and temporal placing of the stage of seizure. They also informed us upon the defects and inadequacies of report in an extremely difficult situation and so led toward great improvement in training and in experimental procedure. A timely shift in method and a frank understanding between *E* and *O* wholly eliminated this error from our main experiments.

Set I. Emoving with Actional Resolution Denied

The predicament was induced by slight electric shock which *O* was led to expect when a continuously sounding tone should end. The emotive occasion was built upon the expectation of a slightly disagreeable instant which *E* was unable to escape by such primitive devices as flight, request, or the use of force. When once 'the malicious, capricious and designing experimenter' had been removed from the occasion, the general setting of the experiment served fairly well as a first approach to the problem. It promised to meet the requirements of constancy, control, repetition and report, the most exacting of all requirements in the experimental study of emoving.

Experimental setting. E was seated in an anteroom before a control-board which governed a timing-device (consisting of brush contact on revolving drum, driven by

constant speed motor and provided with properly timed, uninsulated sectors), a Harvard inductorium, an audio-oscillator, and a microphone-and-pick-up system.[5] The observation began when a tone of 1000 ~ was sent into *O*'s (soundproof) room. The tone continued for 1 min. Concurrent with the ending of the tone, a weakly induced (shocking) current was delivered through electrodes to *O*'s left arm for 1 sec. A periscope with one-way screen allowed *E* to observe *O*.

O was seated, facing the (unseen) periscope, in a comfortable chair in the soundproof room. To his left arm an ordinary dog-collar electrode was attached. On a stand at *O*'s left were a red light and a microphone, with a loudspeaker beyond.

When *O*[6] was seated and the armband attached the following instructions were read:

> You will hear a tone. When it ends, you will receive an electric shock. When the red light flashes, report (into the microphone) in terms of function and of functional product. Report all functioning in order of occurrence, beginning with the tonal signal. Make your reports complete. Do not fail to report all self- and occasional instructions. If the light flashes while you are making your report, repeat.

The preliminary trials acquainted *O* with the method of experimentation and afforded practice in the art of accurate and adequate report. Before the regular experiments began, the following additional instructions, designed to improve the report, were informally placed before *O* for frequent reference.

(1) Are you sure that you are distinguishing comment and flash?
(2) Are all the perceivings which you report actually separate functional performances or are they moments or adjectival phases in something else? . . . If so, what else?
(3) Be sure to indicate perceptive trains when they occur.
(4) Further define and describe what you have called products of inspecting.
(5) Describe what you mean when you report 'relaxation' or 'relief.' Are these body-objects? Are they products of something other than perceiving?
(6) Describe more fully the 'perceiving' of the tone.
(7) Describe further the perceiving of 'shock' and of 'startle.' What do *you* mean by 'startle?'
(8) Are you sure that you are distinguishing perceiving and inspecting?
(9) Are you sure that you are distinguishing inspecting and comprehending?
(10) You report imaginative anticipating. . . . How is it done? Can you describe it further?
(11) Make a further attempt to give sequence in your reports by saying 'leading to' when that is what you mean, reporting parallel modes when they occur, and indicating independence of one function from another by saying 'period' before going on.
(12) Localize tendinous strains, muscular pressures and the like where they are body-objects.
(13) Make your reports fuller and more exact.

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[5] The apparatus was designed and arranged with the assistance of J. D. Coakley. The standardization of the shocking current was difficult, since resistance varied not only from day to day and with the location of the electrode on the arm but also from observation to observation and from *O* to *O*. Thus a given current reported as a slight shock in one observation might be painful in another. To resolve this difficulty the inductorium was so set for a given period of observation that *O* preliminarily reported a slight, barely painful sting. Many preliminary trials fixed this setting at 6.25-6.50 (on the inductorium scale) for all *O*s.

[6] The observers were Dr. T. A. Ryan (*Ry*), Dr. M. S. Ryan (*Sh*), C. Chrisof (*Ch*), J. K. Macmillan (*Mc*), and I. J. Lacey (*La*).

RESULTS

In the present set, 150 regular observations were made. The reports made it evident that the main emotive patterns evoked were two, each presenting variant sub-forms. The first pattern shows the general sequence,

Predicamentive occasion > seizure > decline.

The second sequence reverses two of these stages, thus,

Seizure > predicamentive occasion > decline.

Although the two sequences appeared irregularly and under the same formal instructions, we shall separately consider samples of each.

The first pattern: PO > S > D. The following reports exemplify the sequence where the predicament precedes and leads into seizure.[1]

Pant (tone) > OI (stop what doing) > Imant (vaguely of something to happen) . . . *cum* Eint (pressures in chest; tone; arm as above chair; arms cold; ears ringing) . . . per (slight prick in arm) . . . Eper (body relaxing; disappearance of tensions) *Sb*-5a

Pant (tone) > OI (stop what doing) > Imant (something to come) . . . *cum* Eper (tone in ear; pressure in chest; visceral tightening) . . . insp (tightened stomach muscles) > per (arm tingling). . . . Eper (tightening in stomach; muscles all relaxing) *Sb*-2b

. . . Pant (tone clearer and louder) *cum* Sg (straining forward to end of tone) > Imant (end of tone; shock) > Pant (having lasted long; end in sight) . . . per (body-objects; visceral tightening; tensions in arm; rapid heartbeat) . . . per (relaxing) > SI (get report ready) *Ry*-1c

Pant (tone) *cum* fl ('there it is') . . . insp (same tone) > Imant (shock coming) . . . *cum* Eper (visceral state upset) . . . Eper (right hand; arm; heartbeat; abdominal contractions) > . . . imag (arm being intensively shocked) > rem (L's work) *Mc*-2a

Pant (whole experimental sit'n; room; equipment) > rem (previous experimental sit'n) > Imant (shock to come) . . . per (body-objects; tensing of whole musculature; difficult and heavy breathing). Sp (straining forward without goal) *cum* com ("When will it end? Must come soon.") > per (body bracing to resist) > Eper (relief) *Cb*-4a

A functional analysis of these reports can best be undertaken by dividing the performance into its stages and carefully describing each stage in its turn.

The predicamentive occasion is devised by the organism through an anticipative cast thrown upon the functional trends in course. In this first emotive stage, perceiving, imagining, searching, comprehending or remembering may take this forward-looking cast, enabling the organism to transcend the actual 'present.' Just as in acting, where *the task* falls within an anticipatory phrase, so in emotive predicamenting is there a futural

[1] A list of the abbreviations used will be found on p. 170 f. above (C. Chrisof, The formulation and elaboration of thought-problems). The use of *ant* for 'anticipation' or 'anticipative,' should be added. The term denotes both a separate function and a functional coloring, as of imagination (*i.e.* Ant and Imant).

reaching out, which is, however, anticipative in a peculiar and unique way. Let us see whether we can define more closely.

The apprehending of the tone provided for in the formal instruction was more than the mere perceiving of a sound. There was signalized either the beginning of an uncomfortable episode, or the opening of a period to end with a specific body-object (pricking arm), or, finally, an ominous and unescapable occasion. Like the first element of a known melody (which is really apprehended with a coefficient placed in time with respect to subsequent elements), the aural occasion was here sharply defined as marking the initiation of a highly specialized phase of psychological activity and leading on anticipatively to an undesired outcome.

The first specific means which we here discovered were the *flash* and the *comment*. Here are instances taken from the first stage:

P_{ant} (tone) *cum* com ("I must listen") > fl (it is coming) > . . . *Sb*-1e

P_{ant} (tone) > fl (it is crowding on) > P (body-state: chest presses) . . . *Ch*-6d

P_{ant} (tone) > fl (it is soon to end) . . . *Ry*-2d

Both flash and comment are ready and compact functional resources helping to pack the awaiting-period with predicamentive foreboding. It appeared at times that condensation went still farther to a simple perceiving of 'this is the going-on-to-the-inevitable-prick tone.'[8]

Remembered objects and occasions themselves—strange as it may seem—sometimes acquired futurity stamped by a foreboding expectancy and thus set the stage for an oncoming seizure. A daily instance is the remembered insult which contributes to an onrushing resentment. Under our conditions, remembering was always directed upon situations analogous to the present, which it significantly supported. It entered, as an integral moment, into the predicament.[9] Here are experimental instances:

Rem (previous shock) > Im_{ant} (dread of coming shock) . . . *Ch*-2b

Rem (shock in last obs'n) > Im_{ant} (shock to come) . . . *La*-1g

Rem (ref. to previous obs'n) > Im_{ant} (shock going to come soon) . . . *Ry*-2a

R_{in} (of shock last obs'n) > Im_{ant} (shock to come) . . . *Sb*-3c

[8] W. S. Neff found (this JOURNAL, 49, 1937, 376-418) several stages or degrees of perceiving leading toward a quick and bare abstractive form which was quite remote from the more primitive, concrete and 'pictorial' kind. Looking in another direction, M. L. Lemmon (*ibid.*, 51, 1938, 304-356) noted perceiving thinning out, without accessory processes, into a symbolizing or figurative variety.

[9] Most of our revised and 'prospecting' memories fall under Erb's Type i and Type ii (M. B. Erb, The memorial forms of apprehending, this JOURNAL, 49, 1937, 343-375). Type i: "An occasion or event which bears a particular and explicit temporal and autobiographical reference"; Type ii: "A specific event or scene which bears a vague and indefinite temporal and autobiographical reference." We also found a memorial *flash* resembling Erb's Type iv.

The anticipative form of imagining plays a very important rôle in emoving of the present pattern. It is a sort of matrix into which plain perceiving (or remembering) of the present (or of the past) occasion is imbedded. While remembering by itself may advert to like former occasions and perceiving impart a central significance to the immediate occasion, anticipative imagining frequently incorporates the entire train of functional events and yields a prophetic product.[10]

One further moment, the most difficult of all for reporting, must be considered before we are done with the first, predicamentive stage. It concerns means used by the organism to produce the 'foreboding' aspect of anticipation. Over and above remembering, imagining and perceiving of the sound and its sequels, body-objects, body-states and body-events play their important part. We consider them as well-defined or obscure perceptive products. In the following examples, we indicate them by P_e, where the subscript suggests the peculiar 'emotive' smart, ache, or dread which qualifies perception.

Im$_{ant}$ (twinge to come) *cum* P$_e$ (heavy chest: aching arm) . . . *Sb*-2e

P$_{ant}$ (approaching twinge) *cum* P$_e$ (diffuse body-strains and tensions . . . *Ry*-4f

Im$_{ant}$ (arm-twinge) *cum* P$_e$ (visceral and respiratory events . . . *Mc*-5f

At times, this moment was qualified by a SI which intended the abolition of the 'emotive' body-objects, etc.:

P$_{ant}$ (arm-twinge) *cum* SI (relax and cut out the dreading anticipation) . . . *Ry*-2e

At other times, the mere product 'rising dread' appears in the report without the somatic components.

And still again, this moment in the predicament appears to have been prepared for by a bare antecedent searching (S) set up by warning signs before ever the tonal sound comes:

S (restless foreboding, unattached to any per or im) > P (the tone) *cum* S (as earlier) > Im$_{ant}$ (it is coming) . . . *Ch*-1b

Or the antecedent searching may be supported by a bit of free imagining or remembering. Thus:

Im (looks of E as he sits in the outer room preparing the shock) *cum* S (foreboding) > P (the tone) > Im$_{ant}$ (this will be painful) . . . *Mc*-3b

and

[10] This anticipative moment has been studied by one of the present writers (this JOURNAL, 51, 1938 253-260) as it appears in the actional forms of executive function. In the present shorthand, the moment is set down both as P$_{ant}$ and Im$_{ant}$ according as it inclines more to the perceptive or the imaginational side. In the imaginational form, the presentness of the occasion is nearly or quite suppressed. In extreme cases, the perceptive form, on the other hand, may bring back to the apprehended present the shock-prick before the shocking circuit has actually been closed.

Rem (the last was severe) *cum* S (restless foreboding) > P (the tone) > P_{ant} (the twinging) . . . *Sb*-4c

Finally, the predicamentive stage sometimes led to the first part of an action, which was designed for an escape but was followed by a SI to suffer the inevitable.

. . . Im_{ant} (it is coming) > act (pull away the arm) > SI (keep quiet) . . . *Mc*-4d and *Sb*-3i

The seizure. The outcome and consequent of all that has preceded in the formation of the predicament may be grossly described as a baffled agitation. In many common forms of emoving this second period runs over again into the proposing of means for removing the predicamentive occasion or the seeking of an escape from it. Under our conditions, which tended to exclude the third (actional) stage, the baffled agitation or 'seizure' is nevertheless left. We may not conclude, however, that seizure here would be the same as a seizure which led quickly into an actional attempt at resolution. Let us see what character our seizure possessed and how it came out of the preceding stage which we have just now tried to member and depict.

There are two general characteristics of the predicamentive phase which should throw light upon seizure; the first is its *occasional quale* and the second is its *anticipative trend*. It is primarily a confronting occasion, an occasion to be feared, resented (as by angry rejoinder or reproof), rebuffed, suffered (as by embarrassment, humiliation, self-condemnation or confession), contended against, and so on.

Many occasions are problematical but not emotive or predicamentive. Some carry within them the promise of resolution by thinking (thought-problems), and others resolution by doing (action-problems). As all three sorts of problems are related through many intermediate forms, they may be regarded as forming the great psychological family of problematical functions.

With respect to the second characteristic of emoving, we may observe again that all the great problematical functions are anticipative; all look ahead and all alike advance toward solution, remodeling, or defeat; but each in its own way.

Now 'seizure' also stands related both to an occasion and to anticipation. The ardent thinker knows the frenzied state of an exciting problem, and he has likewise his prophetic moments when the end is in sight and the thought-answer lies on the tip of his tongue. So also with the actional occasion and the actional advance which runs ahead of accomplishment and forecasts the completed deed. Our main interest, however, concerns the active relation of *emotive* seizure to the anticipative occasion which precedes it.

Since it is peculiar to the emotive occasion that it is a bafflement, a situation which offers no immediate release and still cannot be immediately dropped, we might well expect a further demand to be made by the organism upon its general resources and reserves. Again, this demand is by way of being met as emoving proceeds by virtue of the anticipative trend of physiological and psychological function. That which is forecast but not completed in the predicament is bound to color the succeeding stage of seizure.

We shall find, as we might well expect, that the demanding occasion and the forward-running, but unfulfilled, anticipation lead on to a general somatic extension and intensification, precisely to those factors which would create a wide variety of body-objects and body-events.[11] Here are evidences—abundant in our reports—from the stage of seizure.

. . . P. (shallow and rapid breathing, heavy heart-throbs, abdominal strains) . . . *Sh*-2b

. . . P. (tenseness of arms, clammy palms, hot wet forehead) . . . *La*-2h

. . . P. (pulling up in visceral regions of abdomen) . . . *Mc*-3c

The many and diverse body-products recovered by report doubtless owe their somatic origins to processes of respiration, circulation, digestion, secretion, transpiration, contraction, and so on, which are under neural incitement, but which are, outside emotive seizure, either too weak or too much employed otherwise to enter into the psychological functions here considered and reported.[12] Since kinesthesis and coenesthesis are both far commoner than our emotions, it looks as if their emotive contribution depended primarily upon a particular physiological and psychological setting. We should therefore carefully consider them as moments in a total time-course of quite specific construction.[13]

Our experiments strongly suggest that—to put the problem in more concrete terms—the seizure-phase of emoving (in the type of emotive sequence which we have thus far considered) is an inevitable sequel to the baffling and forward-tending predicament upon which the emotive product

[11] Here a physiologist's account of events which did not confuse bodily description with a gross depiction of psychological products would greatly clarify our problem. Nothing satisfactory is at hand. The theory, recently popular among physiologists, of adaptive secretions which augment bodily energies has never been successfully applied to emotions at large. The choice of anger as a typical emotion is not impressive since the alleged 'angry' states fall much closer to a special class of *actional* functions calling for excessive strength and agility than to anything which can sensibly be called emotive.

[12] For a fuller account of body-objects and the like, see H. C. Werner, A descriptive study of somatic objects, this JOURNAL, 51, 1938, 225-234.

[13] This view stands of course against the neurologist's view of emotion as fixed and determined by certain cortical and subcortical centers and peripheral organs and contrivances.

is based. The phase is greatly enriched and widely extended in specificity by the somatic components which we have cited and which appear both in vulgar recitals of emotional seizure (the straitened chest, the dry mouth, etc.) and in trained report. To extract these components one by one, to set them down as discrete sensations and feelings making up the state, or to regard them as creatures of the hypothalamus and the autonomic, is to make a caricature of the type of function which we here inspect. Even in forming as careful a functional report upon this difficult matter-of-fact as the trained psychologist can command, there remains the danger that such an item as 'catching the breath' or 'clenching the fists' will be over-interpreted as a separate or separable moment in a panicky-dread-of-being-pained.[14] Such items are undoubtedly actual, but they must be set into the context and regarded as a part of the integrated functional flow.

In divining the real part which these moments play in emoving at the seizure-stage, we must make allowance for the analytical effect upon the function from inspecting, whether inspecting results from an instruction (formal or self-initiated) to 'note critically' or from the sheer impressiveness of such symptoms as the trembling of terror or the aching set jaw of impotent rage. At times these body-objects are powerful enough in their own right to cut across and even to dissolve the predicament. The case is just the same as the noting of the headlights of a following automobile when the light shed on the road ahead grows sufficiently strong. The notion that bodily postures, muscular turns, and 'vital twists' serve as 'gauges' and 'indicators' to inject flashes of doubt, denial, query, certitude, encouragement, and the like, into elaborative thinking will serve us also in emoving. In so far as these somatic products are compacted into the bodily resources of the seizure, so far do they tend to disappear as inspected products of perceiving and to contribute to the entire functional trend of the moment and to its single psychological product. To them we may then extend the term 'indicatory transfers.'[15]

Decline. In our experiments, the pungent twinge in the arm from the electric current is the end toward which the searching, anticipating, and all other functional events are aimed, but it by no means marks the end of emotive functioning, which does not immediately vanish with this per-

[14] The danger is still greater in neural physiology where the naïve notion flourishes that a block of these somatic processes, taken together with their central inciters and sequels, *is* the emotion.

[15] For the primary references to such indicative or gauging moments in elaborative thinking, see M. Bentley, *The Field of Psychology*, 1927, 355-357; *The New Field of Psychology*, 1934, 305. Further experimental study of these subtle but important moments in the emotive course is urgently called for.

ception. Actually this body-event is no more a 'mere perception' to *O* than the sounding tone was. It is something like a 'there-it-has-come-at-last: now-it-is-over.' In other words, this event marks a new turn in the emotive course. It begins the stage of decline. Seizure now fades toward a memorial echo and new turns of bodily function give coloring to 'relief.' Inspection at this point reveals the body-products that contribute to decline. Among the most prominent items are the feel of muscular and tendinous relaxation, with constant changes in respiration, heart-beat and general posture. The same 'transfer' or 'translation' out of perceived body-objects and events and into 'decline' that we found in seizure occurs again here. The great concurrent change in psychological functioning is the disappearance of the anticipatory reference and all that it contains.

For this stage we have the following excerpts from reports:

. . . Per (relaxed trunk and arms) > fl (there it is) *cum* com ("it is ended") . . . *Sb*-2d

. . . Rem (the tone *was*) > per (dropping in visceral region: arms relax) . . . *Mc*-3b

. . . Insp (tingling in arm) > comp (tingling was akin to tickle); com ("I must report") . . . *Ry*-2b

It is obvious that such reports as these are inadequate to the functional significance of the declining phase of the emoving. The main psychological product ('predicament is no more: this tingling sharpness ends it and absolves me') tends to disappear behind the inspected details.

Modal variations. In a fairly large number of cases the fundamental mode *po* > *s* > *d* suffered internal changes. The simplest change was the reinstatement of a second predicamentive occasion just after seizure. At times this reinstatement led to a second seizure, and at other times to further repetitions of the double phase *po* > *s*. These variants suggest, concordantly with daily experience, that the disturbance in functional course easily leads to a baffled alternation of phases in which the unsettling occasion may arise again and again, thus augmenting and prolonging the seizure.

The second emotive pattern: S > PO > D. All men know this form of emoving, which frequently begins with a violent automatized movement of the body and of its members, as from a sudden loud noise for which the organism is not immediately prepared. Seizure strikes; to be followed by a perception or understanding of a predicamentive, usually a dangerous or annoying, occasion. This mode of reversed phases appeared in our experiments when the tonal sound acquired a 'startling' character which immediately evoked the seizure before any corresponding occasion was fabricated.

The following extracts will serve to show the order:

P_o (tone: start of body forward; pressures in chest and ears) > Im_{ant} (something coming) . . . *Sb*-1d

P_o (tone: muscular contractions in abdomen; eye twitching): P_{ant} (tone) > Im_{ant} (shock coming) . . . *La*-2c

Modal variations. Again the swing between first and second phases, sometimes with a second seizure following the tardy occasion and sometimes with two or more alternations in sequence before the final decline. Another variant entirely omitted the predicamentive occasion, decline immediately setting in after the initial seizure. Possibly this should be regarded as the simple startle, though everyone who suffers from this hot and unpleasant sort of upset-without-warning will agree that it usually possesses a decidedly emotive coloring. Wherever a human perpetrator of the 'startle' is suspected, the sufferer is inclined to fill-in an intermediate phase of predicamentive occasion charged with helpless resentment against the annoyer.

Set II. Emoving with Actional Resolution Encouraged

The experiments next to be described were designed to provide a predicamentive occasion which *O* might resolve by way of an appropriate actional train. The procedure accordingly arranged for the insertion of a penultimate stage, *attempt at resolution,* in the functional performance. Our central observational task was to be the careful description of this penultimate stage as it grew out of the preceding predicament and seizure. We were to discover, however, that the experimental setting and character of the task assumed by *O* greatly changed the entire performance, both in complexity and in functional temper.

The task set by *O* for solution was the picking out on the musical keyboard of a certain melodic fragment which would prevent the sharp body-tingle event from the electric shock, otherwise to be administered to the arm.

Experimental setting. The general arrangement was the same as before, save that *O* was seated before the modified piano-keyboard of a reed harmonical. The same tone as before was sounded for 1 min. Thereafter, at a variable interval, the shock might (or might not) be administered. The playing of a familiar melody required no technical ability beyond the picking out of a few initial notes. The melodies to be used were independently submitted by seven persons not related to the experiments. They were to be the titles of songs 'known to everybody.' Those finally selected by *E* had appeared in six of the seven lists. When seated before the harmonical, with the electrode in place on the left arm, several of the song-titles, clearly

typed on separate cards, were placed face-downward in a pack near *O*'s right hand. The formal instructions were given orally:

When the tone begins, you are to play (on the instrument before you) the melody indicated on the topmost card in the pack. You have until the tone ends to play the tune correctly. If by the time the tone ends you have correctly worked out the pattern of the melody, the shock will not be delivered.

When the red light flashes after the tone has ended, make your report in terms of function and of functional product. Your task is to make a complete report, item by item, omitting nothing that falls between the beginning of the tone and the flashing of the red light.

When your report has been received, the red light will flash three times. You are then to remove the top card and to go on to the next in the pack.

E was the judge of the correctness of the performance. He was thoroughly acquainted with the melodies to be played and had had enough musical training to pass upon the value of the playing. *O* was required only to work out the general pattern of the melody to avoid shock. *E* placed a liberal interpretation upon the performance. The shock used was within the range of intensity determined by the standardization of Set I. When the playing was unsatisfactory to *E*, the shock was delivered shortly after the tone had ceased.

In the second half of the set the formal instructions were supplemented as before in order to improve the reports. The supplement ran:

(1) When you report, be sure to title correctly your functions.
(2) Be careful that you do not confuse acting with movement and its issues.
(3) Do not blanket products of other functions under the term 'perceiving.'
(4) Make sure you sharply distinguish an independent function from adjectival coloring.
(5) Describe the stage after the tone has ended more fully than you have done.
(6) Describe fully the nature and composition of 'predicament.'
(7) Describe the bodily aspects of your performing.

The set includes 160 observations, preceded by a training period to acquaint *O* with the harmonical and the formal instructions and also to permit *E* to determine *O*'s skill in manipulation. The intent was to administer the slight shock with reserve and discretion, although it might at any time be anticipated by *O*.[16]

The possibility that the ensuing functions might be actional in character makes a comment on the varieties of action in order. Where the *actional task* was incorporated in our reports, it usually corresponded to Longwell's 'specific' sub-form, referring prospectively to *O*'s manual use of the keyboard.[17] Less frequently appeared Longwell's 'explicit' kind (this particular melody is to be played), and from time to time various 'secondary' tasks (indefinite, definite and elaborate).

First we shall review, as independently as the facts will permit, that aspect of the entire course which introduces the actional task, always remembering that this task is built up and governed quite as importantly by the occasional and self-initiated forms of instruction as from the formal

[16] Illness prevented observer *Ch* from completing the last half of the set.

[17] See S. G. Longwell, Progressive change in simple action; a study of learning, this JOURNAL, 51, 1938, 261-282. The reader who is not acquainted with the executive functions at large will find comprehensive accounts in M. Bentley, *The New Field of Psychology*, 1934, 209-265, 396-400.

variety contributed verbally and otherwise by the experimenter. In our setting, the main occasional instructions came from the apprehensional grasp of room, cards, song-title, harmonical, flashing light, and so on.

Here are samples:

. . . Per (light) > OI (turn card and look) > insp (card title) *cum* fl (familiar > imag (vocal rehearsal) . . . *La*-4a

. . . Per (title) > imag (tune: schematic group of words; further notes; entire phrase): per (tone) > OI (play the imagined product) . . . *Ry*-2c

. . . Per title) > imag (one phase of tune; first note as d'): per (tone) > act (determination to play; playing d') . . . *Ry*-3b

. . . Per (light) > OI (turn card > per (title) *cum* com ("I know it") > rem (occasion this was sung) . . . *Mc*-1c

This phase of the actional mode sets into commission the determination, *i.e.* the carrying forward of the task. In our reports it is opened by imagining, remembering, and other functional moments which point toward resolution, after occasional and self-instruction have set the task. The forward-running determination provides solidity and continuity. Longwell has stressed its anticipatory cast, but it is also necessary to emphasize the manner in which it marshals the products of previous performing and incorporates them into the activity. This is well exemplified in the reports below.

. . . (carrying through OI to play) *cum* imag (of tune running ahead) > SI (find first part of tune) > imag (notes accompanying first words) > SI (play them) > act (playing from d' under SI) . . . *Ry*-2c

. . . imag (of tune) > S (complete the phrase): insp (imagined tune; first note about a' > SI (start on note a'; play without sharps and flats) > (playing of phrase) *cum* insp (almost correct) > SI (insert half-steps) . . . *Ry*-1a

The final phase of the actional moments is resolution of the task through movement. But in our actional trains, while a single task remained, the entire performance was divided into segments, each of which held determinational and resolutional phases. Determination is constantly recast and is followed by new motor activities in each segment. Released and directed by perceiving, inspecting, and searching, each determinational phase runs into a motor activity which is specific.[18]

It is the rule for an underlying determination to be present straight through, representing a permanent general task, but to be repeatedly specified and reënforced.

There were instances where nothing more than a very slight predica-

[18] Bentley has portrayed this variety of acting as Formulation of task > Progress of determination > Resolution and new apprehension > Progress of determination > Resolution and new apprehension > Progress of determination (*New Field*, 217).

mentive shading (anticipative perceiving or imagining before or when the one-minute tone began to sound) was added to the action, which then ran a characteristic course to the successful rendition of the melodic fragment. In these cases, the final resolution of the action did, to be sure, hold a kind of relieved ejaculation which brought the products 'fine'; 'that's right'; 'I've escaped the penalty.' This might be called a triumphal resolution, a triumph really sweetened by the initial foreboding that 'this may turn out badly' or a surrogative remembrance of earlier trials where the melody failed and the twinge fell.

These instructive instances, where only the shadow of an emoving crossed the functional course, require only one more comment, a comment to explain how and when "the slight predicamentive shading" passed into plain and unimpeded acting. It appears from the *Os*' reports that the striking of the first note or notes of the melody suddenly brought the change, and that this change wore a double aspect. On the one side, it characterized the actional determination ('this is the way it goes'; 'I have it'; 'I can do it'; 'aha! I am safe'; 'how exciting!'), and on the other side, it was the fading out of a foreboding uncertainty which nevertheless so lingered as to lend a positive coloring to the first phase of the free action. The whole change in tone of the performance was often sustained by bodily changes which brought (under inspection) such reports as 'quickened breathing,' 'springy thrills through the shoulders,' 'a relaxed throat,' and the like. Those who number *relief* among the emotions would call these bodily objects and events evidences of this emotion; but if we hold to the predicamentive occasion as the core of emoving, we shall have to regard this moment as a sequel to, rather than a constituent of, emoving, *i.e.* as a notice that emoving has come to an end.

Now we turn to those reports which disclose *a really predicamentive foundation.* We here find the activity turning decidedly emotive under three conditions; (1) failure to set the actional task; (2) failure to establish a suitable actional determination; and (3) miscarriage of the manual performance upon the keyboard. The conditions fall thus into the given temporal course, *i.e.* the task is not set; it is set but not translated into a prophetic determination; when set and determined, it miscarries in the stage of resolution.

While we go on to examine these cases, we must keep before us the well-attested fact that an actional performance *may* fall short in all these ways without turning emotive. Take ordinary piano-practice itself. The learner may stumble in a difficult passage (1) through neglect of the teacher's directions for fingering and so miss the task; (2) in his anticipa-

tive attack upon the approaching passage, or (3) through fumbling as he strikes the successive notes. All lead to bad and balked performance, but not one of them necessarily leads to emoving; although any may, indeed, when circumstances (*e.g.* great fatigue, discouragement, or the teacher's reproach) favor. What is the difference? Unless we mean to beg the question by falling back upon the 'emotional situation' of the behaviorist as a causal agent, we shall have to be content for the present to say simply *context* and try later to define what that 'context' is which distinguishes an actional emoving from a disturbed acting.

(1) Failure in tasking. The task, as *O* attempts it, is impossible either by virtue of its difficulty or because it is unfamiliar. The course of events then takes the following form:

Per (light as OI to begin > [card turned] > comp (title unfamiliar) > fl (not going to get this) > Im_{ant} (will get shock) > P_o (visceral stirring; shallow breathing) > R_{in} (for title) > rem (hearing song) > SI (remember the tune) > S (to imagine) *cum* com ("I don't know this") > Im_{ant} (shock) > P_o (highly agitated body-object) . . . *Sh*-4e

Per (title) > com ("don't know this") > Im_{ant} (of shock) > P_o (body-strain objects; difficult breathing) . . . *Mc*-2e

[card exposed] > per (title) > comp (don't know this) > insp (title unfamiliar) > S (for melody) > Im_{ant} (shock to come) > P_o (trembling legs; straining arm-muscles; visceral excitement; time passing; tone-end) . . . *La*-3f

We note that the observation-period contains attempts to formulate the task, the organism calling upon various functional aids. The period is abortive; nothing is accomplished. A tense searching for the melody is set up. *O* is helpless; he awaits the impending shock. The predicament consists here in *O*'s inability, at a certain juncture, to formulate correctly the task and to provide a basis for determination and resolution. We may represent the general procedure as Attempted tasking > Predicamentive occasion > Seizure > Decline.

A variant of this course occurs when brief, ill-founded, and unavailing actional determinations or movements appear in spite of inadequate formulation of task. Such determination and movements invariably lead to a renewal of the predicamentive stage and a more intense seizure. The course is extremely difficult to diagram, since the stages intermingle and overlap.

To exemplify:

Per (card) > comp (title) *cum* com ("My God, I don't know this!") > rem (many tunes) > SI (do something) > Im_{ant} (shock coming) > (random beating at instrument) > P_o (tone end; gripping strain in trunk; heart quickening; intense general strain) *cum* Im_{ant} (of shock) . . . *Mc*-1b

Per (tone) > per (kinetic contraction; straining forward) *cum* [exposing card] > comp (title) > imag (vocal rehearsal) > insp (melody wrong) > S (for rest of melody) *cum* com ("don't know it") *cum* Im_{ant} (of shock) > P_o (general bodily seizure) > SI (take it easy) > S (for rest of melody) *cum* com ("don't know it") *cum* Im_{ant} (shock to come) *cum* P_o (bodily tenseness; trunk excitement; trembling legs) . . . *La*-1c

We all recall incidents where emotive performing has resulted from an insufficiently clarified or formulated task. The actional determination rarely gets under way, and time is consumed in performance directed toward a clearer exposition of the task. Help is sought in remembering and imagining; unrelieved searching strains for the solution, and the sum is a thwarting occasion accompanied by an organic crisis.

(2) Failure in determination. Here an imperfect actional forecast fails to provide for resolution of the task through movement. Instead it leads to an anticipative imagining of shock and to the agitation of seizure. Proposals for movement are considered and rejected. Movement never comes about. The main rôle is played by searching. Imagining is also called upon for aid, often emerging from search. SI is prominent, and the entire course consists of a series of attempts to move forward to the stage of resolution. Out of the unrelieved searching arises a predicament, and seizure follows.

Per (title) > comp (melody familiar) > S_{im} (what *is* the tune?) > SI (get going): > S_{im} (the tune is?) > imag (briefly of first phrase) > S (to confirm imagined product) > insp (imagined product) *cum* com ("Is that right?") > SI (start playing) > S (for place to begin) > Im_{ant} (shock to come) > P_o (general bodily upset; end of tone; body-objects) . . . *Ry*-4f

Per (light) > OI (turn card and begin) > comp (title familiar) *cum* OI (to sing it) > S (for tune): per (tone) > S (for tune) > S (to imagine V singing it) > imag (part of tune) > S (get it placed) > imag (briefly of tune) > S (get melody) *cum* Im_{ant} (of shock coming) *cum* P_o (tenseness throughout body; pressures in chest) . . . *Sh*-3a

The outstanding fact here is that movement never comes off because determination is defective. The formula should stand, then: Task>Abortive determination>Predicamentive occasion> Seizure>Decline.

In a modified form, predicament and seizure are followed by a brief new attempt at resolving, which again gives way to predicament and another seizure. To exemplify:

. . . S (for melody) > Im_a (vocal rehearsal) *cum* fl (not right) > S (again for melody) > Im_a (vocal rehearsal) *cum* insp (right tune) > OI (play it) > S (for beginning) > Im_{ant} (shock coming) > fl (won't get it) *cum* P_o (bodily agitation; palms sweating; heart faster; excited body-object): SI (try it) > [attempts at playing] > Im_{ant} (self-receiving shock) *cum* P_o (heart fast; pulse fast; sweating) . . . *La*-3e

. . . S (to imagine tune) > SI (I get it right) > SI (imagine) > S (to imagine) > Im_{ant} (shock at end) > P_o (shallow breathing; pressures in stomach; heaviness of

limbs; tensing muscles): > SI (try to play) > (random striking at keys) > Im$_{ant}$ (shock is coming) . . . *Sb*-2e

. . . S (to identify first note) > SI (imagine first note) > S (for note) > Im$_{ant}$ (shock coming) *cum* com ("I won't get this") > P$_{o}$ (muscular tension: trunk-objects) > SI (play) > S (for first note) > SI (hurry and play) > S (for first note) > Im$_{ant}$ (going to get shock) *cum* (excited body-object; muscular bracing for shock) . . . *Ry*-3e

Note the use here of powerful self-instruction to destroy the predicament and to restore the actional course.

(3) Failure in resolution. This is the commonest variety met in our melodic set. Task and determination are both present, but the movements of resolution are ineffectual. Let us examine examples:

Per (title) > imag (melody) > S (to imagine clearly): per (tone) > SI (play intervals) > [playing intervals] *cum* fl (wrong) > [playing other intervals] *cum* fl (wrong) . . . [playing] . . . comp (can't play tune) > [striking out at random]: > per (end of tone) > Im$_{ant}$ (coming shock) *cum* rem (poor performance) *cum* P$_{o}$ (train of body-objects) . . . *Ry*-4b

. . . [picking at keys] *cum* fl (not right) > SI (go on) > S (to imagine) > SI (pick it out) > [playing] > fl (wrong) > [random picking at keys] *cum* P$_{o}$ (intense bodily agitation; body-objects) . . . *Ry*-4c

. . . [pressing keys] *cum* OI (lower) > [pressing lower key] > insp (right) > [playing] *cum* insp (wrong) > [playing]. per (end of tone) *cum* com ("that's over.") > Im$_{ant}$ (shock coming) . . . *La*-3g

In the first report the task is correctly formulated and unhesitatingly accepted. Task enters by way of formal and occasional instruction and is given aid and clarification by the products of imagining, searching, and inspecting. The actional determination is consistent and persevering. Frequent self-instructions (to continue, to get it correctly, to make other attempts) color all the reports, while forward-tending searching assumes a prominent place. The movement resolution, however, is at fault. *O* discovers (fl), by way of perceiving and inspecting, that his playing is incorrect, and this leads him imaginationally to anticipate shock and to suffer seizure. In many cases the breakdown of the performance is so complete (as in the first two reports above) that *O* hits out at random, indicating a complete disintegration of the coördinated actional train. Because these attempts at resolution prove inadequate and deficient, therefore, the emotive outcome follows. The crucial point—and this holds generally for actional emoving—lies in the functional setting. The apprehensional type of anticipation found in our non-actional emotions of Set I is here wanting; nevertheless there is about the present performance an aura of futurity common to all incomplete acting, an aura which derives from the determinational provision for resolution through movement. The resolution may fail, in our experiments, either because *O* lacks skill and

musical knowledge necessary to pick out the notes or because the occasion suggests an impending shock-object at the arm. In the first instance, the actional problem simply ends; in the second, the occasion sets up its own futurity which cuts across and disorganizes the action. Acting then disintegrates by virtue of the competitive train of bodily and psychological events leading toward the impending twinge.

The key to the emotive functions. Before we conclude and summarize our studies, we should relate them to the general field of research in which they lie.

There can be no doubt that seizure is the most impressive aspect of our emotive experiences. Impressive it is whether viewed as the spectacle of a hominid or other animal exhibiting violent, agitated, or unrestrained behavior; as the trembling, throbbing, muttering and perspiring of one's own body out of control, or as a shorthand means of indicating or expressing to another one's undone and agitated states. Nevertheless, our attempt to regard emotion as one of the great psychological modes of bodily performance, arising under assignable conditions, taking a specific course, and ending with inevitable issues, has led to experimental results which make *the predicamentive phase basal and seizure secondary and derivative.*

The history of the subject, from Darwin, Lehmann, Lange, and James to recent times, points unfortunately to seizure as the characteristic of emoving that has almost wholly determined theory, doctrine, description, classification, and research. Given this natural, if thoroughly naïve, absorption in seizure, it is easy to discern two major interests of the last seventy-five years which have been mainly responsible for our emotive history. The one interest derives from the biological theory of adaptation (what *is* in behavior exists because it has usefully conformed the living being to life's necessary conditions); the other interest derives from morphological theory (life depends, individually and racially, upon the organism's structure and configuration). The first interest interprets the processes of life, the second its physical constitution.

Darwin emphasized the first in his contention that the outward evidences of seizure give the clue to many behavioral resources of the body. Thus his 'laws of expression.' Those in search of body-seats of emotion, on the other hand, have delved—like ardent phrenologists—into thalamus, hypothalamus, adrenals, and autonomic, to discover the secret place of seizure. Lehmann and hundreds of his followers have tried to catch on graphic record and oscillographic display, the operational drift of seizure in muscular, respiratory, circulatory, cerebral and glandular processes.

Lange stood especially for circulatory and respiratory causes, while James emphasized the skeletal muscles. With his interests and sympathies divided between physiology and folk-beliefs about the mysteries of life, James capitalized the dramatic possibilities and shocked the psychic causalists by supposing that the physical states might underlie seizure and so *cause* the emotion as felt or suffered, instead of symptomatizing or 'expressing' it. The effects of this resounding casuistry would never have echoed through Europe and America for a half-century had anyone ever discovered what emoving actually is; discovered, that is to say, as taxonomist, astronomer, physiologist, embryologist, and the comparative student of languages discover the kind, character, and course of the objects and processes which they examine.

Summary and Conclusions

Our pioneering studies of emoving have sought an experimental approach to one of the main psychological modes of operation of the living body. Always we have taken advantage of prior descriptions of those forms of psychological activity which are better known, especially perceiving, remembering, imagining, acting, and searching. Furthermore, we have constantly found indispensable aid in a comparative examination of the three great 'problem-solving' functions of acting, emoving, and elaborative thinking, for underlying all these are pronounced and significant similarities.

Finally, the present chaotic state of our subject, in whatever context it is examined, has admonished us to keep in mind the general historical setting. The main facts of this setting we again review for the sake of our summary and interpretation.

The historical approaches to the study of emotion, distinguished in our introductory paragraphs as physiological, pathological, theoretical and expressive, show the very great influence exerted in each and all by the powerful doctrine of biological utility and the ancient assumption of inherited local bodily seats. Physiology and pathology have generally stood for specific processes, normal or diseased, running their singular courses in fixed and local parts and organs of the head and trunk; the more speculative theories have held that the emotions are either sheer functional breakdowns or else veiled devices like instincts, desires and habits bestowed for purposes of adaptation; and the expressionists, setting seizure in the midst, have made the pantomimic display of posture, pose, mien, and facial cast the 'expressive' indicators of this central feature.

That no one of the four main approaches leads toward a really psychological description of the emotions should be obvious from the direction of their incidence as well as from the issues of many decades of research undertaken since Darwin wrote his *Expression of the emotions in man and animals.*

The study has shown, as we think, that it is possible to secure a descriptive (we should not say a 'photographic') account of at least certain of the emotive functions of the body with precisely the same empirical methodology as has been successfully used in all of the other primary operations. The method implies special training, a technical vocabulary of the operational or functional sort designed for events in course rather than for fixed objects, and a disposition to avoid doctrines and explanatory conceits derived from other disciplines, notably from speculative neurology and biology. While its own description is not made in terms of anatomy or of the physiologist's processes of the body, the method finds a place for all *factual* accounts of the living organism which bear upon the psychological functions. These bodily facts are of the first importance once the psychological operations have been described, because only bodily agents and forces (not psychical or mental mechanisms) are to be admitted as terms of energetic explanation.

In our first set of experiments we examined a type of emoving which was as remote as possible from acting; while in the second half emoving appeared in many instructive admixtures with closely related actional forms.

The main issue of Set I was the discovery of the nature of the predicamentive occasion (the pivot of emoving) and of its functional relations to the stage of seizure. While the experimental situation led to variable ligations in time as between these two emotive phases, it has appeared that in the complete-in-itself emotive course the organism characteristically involves itself in a dramatic way through the creation of the predicament, and that the predicament resolves itself into the consequent phase of seizure, which, in its turn, suffers a natural decline and gradual obliteration.

Since it is of the functional essence of emoving that the demands of the predicamentive occasion cannot be satisfied, as the actional problem is, by appropriate movements of body or limb, we sought in Set II instances where a train of more or less resolving movements entered to temper the emotive course. In some instances, emoving was practically eliminated by the prompt playing of a melodic fragment suggested in the presented song-title; practically, but not wholly, for always there was the foreshadow of the stabbing twinge which meant that the playing of the

notes might not reach its goal. In other instances, there was more of a demanding and dramatic occasion, which set deeper the emotive coloring of the performance.

Here our most important problem was to see how the emotive function distinguished itself from the actional. The solution lay in those turns and shadings depicted in our reports which made the primary occasion predicamentive. The anticipative moment is obvious. We have noted the 'aura of futurity' in the occasion; but this moment is more than a simple prophetic forecast which also attaches to the determinative aspect of acting. The occasion rather becomes a future in which the emotive observer is implicated as by a threat. The event *impends* in the sense that it holds a foreboding for the individual. That is to say that it holds a certain inimical value for the self-object.

Taken all together and set against the extensive and significant background of history, the experiments here written down have directed the present writers toward three main conclusions.

(1) The first conclusion is that the subject of 'the emotions' may well be taken from the half-explored region of arid and frozen *things*, physical and mental, and concretely examined as a kind of *event* among other events through which the living organism constantly moves. So considered, emotion appears as an emoving, *i.e.* as a specific and well-marked function bearing a striking resemblance to the other resolving functions of elaborative thinking and acting.

(2) The second conclusion is that emoving may properly be called 'psychological' both from the nature and course of the event itself and from the character of its issues and products.[19]

(3) The third conclusion, drawn quite directly from our observational reports, is that no single item, detail, feeling, stimulus, or response is to be identified with emoving, which is an entire integrated train. This organized train it is which makes the tone-heard, the anticipative imagining, the searching, the flashes, the signals, the blundering movements over the keyboard, and all the other details, really emotive.[20] This is what we have meant by 'context.' It is as much a caricature of emoving to resolve it into a perception or a feeling as to dismember it into a 'sensation with its aura of affect,' to name it a response to a situation, to confuse it with assumed

[19] When 'products' have been confused with inciting conditions the whole problem of emoving has miscarried. To speak of an 'emotional situation' (a thing to be created by the organism) as the *cause* of an 'emotional response' is a plain *petitio*.

[20] No demand is here made upon *Gestalttheorie* and other popular doctrines which elevate totality and the unitary aspect of things into an explanatory cause.

subcortical connections, autonomic systems and visceral violences, or to identify it with 'expressive' movements and neuro-electrical potentials.[21]

These conclusions imply neither a classification of emotions at large (a minor and secondary problem) nor a general survey of the emotive field. Most of the competent research in both of these directions remains to be done and done with better methods and a clearer envisagement of problems than the last half-century has provided.

[21] An adequate description of the bodily resources and sequels of emoving remains to be written. It can hardly be written until an empirical account of emoving itself has been taken over and understood by physiologist and pathologist. Competent descriptions of bodily organs, systems and processes, without an appeal to the frozen entities of the emotions, are another matter. They would be an accomplishment of the greatest service to the psychological student of emoving.

THE PSYCHOLOGY OF SOCIAL CULTURE

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A decade ago it appeared that social psychology, under the influence of behaviorism, had finally attained scientific status as an explanation of social interaction in terms of stimulus and response. Among the laws and principles which it employed to account for social behavior, the chief were those of conditioning, particularly to social and symbolic stimuli, and of habit formation. Closely related to habit, and in some respects scarcely distinguishable from it, was the conception of social attitude, a generalized habit of preparatory neuro-muscular adjustment to a social stimulus operative to a large extent through the vocal symbolism of speech. Focussing as it seemed to do the fundamental principles of behaviorism for an attack on the problem of the everyday interactions of individuals in society, the concept of social attitude became the leading theoretical tool of the social psychologist, and experimental investigation in this field of recent years has consisted mostly of an attempt to measure and define social attitudes. This investigation has brought to light some very interesting facts, interesting and no doubt important in many cases, but whether the results have been commensurate with the energy expended remains an open question.

There is no doubt, however, that the application to the social field of the principles above-mentioned, and others logically linked with them in the behavioristic approach, has greatly enlarged our understanding of certain departments of social behavior, notably the initial stages of learning and the routine adjustments of orderly social life. But they furnish no explanation for other features of social life and conduct held by earlier social psychologists to be distinctive and outstanding. Such features, for example, are the participation by individual members in an inclusive social, or group, experience whose comprehensive organization is in some degree reflected in the experience of the associated individuals, and the influence and authority of social ends or values in shaping the opinions, determining the decisions, and coördinating the activities of individual members. One need not wish for a return to this earlier approach, or for a reinstatement of the social mind or consciousness in this branch of psychology, to be convinced of the importance and authenticity of these features of social life. Hence, one may welcome as a sign of real progress the

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emergence of new facts and new views which indicate the importance of immanent order, organizing values, and functional relationship in the field of social life and behavior, and demonstrate the inadequacy of the theoretical framework of stimulus-response behaviorism to account for these features.

It will prove helpful, therefore, to remind ourselves at the start of some of these contemporary advances in socio-psychological theory which bear directly upon the psychology of social culture.

By his studies of the mental growth and social relations of children, Piaget[1] has shown that intellectual development and social adjustment go hand in hand: the child passes from a primitive ego-centrism in which he perceives and imagines things only in relation to his own actions and interests to that of social objectivity, by the discovery of logical relations between things and classes of things which coördinate the perspective of different individuals in the social and really objective world. It is not without significance, furthermore, that concepts of psychoanalytic theory, like identification, introjection, and projection, are winning their way to quite general acceptance among social psychologists,[2] despite the fact that they are not easily reconcilable with behavioristic interpretations. This is because, besides being supported by clinical findings, they give genuine assistance in explaining the social adjustments of infancy and early childhood.

The chief contribution of the Gestalt school to social psychology lies in the development of the field of theory of social behavior by Lewin[3] and Brown.[4] The basis for this theory is found in the alleged fact, for which these psychologists have gathered a considerable body of evidence, that when dynamic or motivational attitudes are aroused in the field of social behavior, an object present in the situation of an individual and related to some organic need arouses in him a tension which is not merely intra-organic but directed outward in a line of stress upon the external object, and that this goal-directed tension determines the grouping and organization of objects in the individual's total field of perception and action. Not unrelated to this insight is the view recently advanced by F. H. Allport[5] that the behavior of the individual (as an expression of his personality which has always a social reference) is best understood when described teleonomically. This is the describing of an individual's behavior in terms of the purpose or purposes he seems to be carrying out, of what he seems to be 'trying to do.' Allport is explicit in denying that this implies teleology in the ordinary sense of postulating an end or goal antecedently in the consciousness of the individual which impels him to acts that accomplish some result beyond the acts themselves. The end or goal in terms of which it is proposed to characterize the act has no place except in the act itself. In making use of such description, we mean only that if we were performing the

[1] T. Piaget, *The Moral Judgment of the Child*, 1932, 1-418; Principal factors determining intellectual evolution from childhood to adult life, in *Factors Determining Human Behavior*, Harvard Tercentenary Publications, 1937, 32-48.

[2] G. Murphy, L. B. Murphy, and T. M. Newcomb, *Experimental Social Psychology*, 1937, 189.

[3] K. Lewin, *A Dynamic Theory of Personality*, 1935, 1-286.

[4] J. F. Brown, *Psychology and the Social Order*, 1936, 1-529.

[5] F. H. Allport, Teleonomic description in the study of personality, *Char. & Personal.*, 5, 1937, 202.

movements made by the individual we should say we were doing thus and so. Since the words which the agent speaks are a part of his total behavior, these may be used as evidence of the correctness of teleonomic description, not of course at their face value but in relation to the environmental circumstances and the rest of the individual's behavior.

Value, whose significance, says the writer of a currently published work on the psychology of personality, "has been ignored by psychologists until relatively recent years,"[6] is given a central importance by Sherif who treats the customs, laws, traditions and standards of society as socially established values.[7] Social norms or culture-products are, as he points out, at first external to the individual and act upon his organism from without, but not as discrete sensory stimuli present and operative among those furnished by successive social stimulus-situations. Such situations are always perceived as relational wholes, the result of the organization of external and internal factors of stimulation. Social culture-products also—language, music, architecture, furniture, tools, etc.—have a structure, an order and organization of their own, which is directly perceived as real.[8] Understanding of the meaning of such objects, at first only partial, increases rapidly in the first years of life. As the result of responding thus to the culture-products dominant in his environment, the individual interiorizes the norms of his society. Once appropriated they exert, as Sherif emphasizes, a constant influence upon his perceptions and attitudes as 'frames of reference,' *i.e.* internal factors which to a large extent predetermine the relationship and context of things as they appear in the field of perception and action. Agreeing with Sherif in fundamental principles L. B. and A. G. Murphy[9] have attributed the sterility of much research on the social behavior of children to the 'atomistic' approach which has treated the child's act as a response, isolated from the child's total personality and detached from the situation in which it occurs, to a stimulus standardized for uniformity in experimental procedure. They recommend as the only valid procedure the method of functional analysis which recognizes the high degree of interdependence of situation and response, and defines behavior traits not as response elements but as aspects of the responses of whole organisms to whole situations.

Anthropological research has lately established the fact that the various elements in a social culture—the different institutions, traditions, magical and religious rites, art-forms, etc., comprised in it which had been previously studied singly by the comparative method—prove when examined in their relation and as composing the total culture of a particular group, to be functionally interdependent and to constitute a logically and practically organized system. The social (and psychological) import of these discoveries is indicated with due scientific caution by Malinowski.[10] All the elements in a primitive culture, he affirms, prove on investigation to be functionally related. These are no idle survivals or disconnected traits; in even the most queer and sordid customs there is a core of rational and social

[6] R. Stagner, *Psychology of Personality*, 1937, 181.
[7] M. Sherif, *The Psychology of Social Norms*, 1936, 1-210.
[8] *Ibid.*, 32, 56 ff.
[9] L. B. and A. G. Murphy, The influence of social situations upon the behavior of children, in *Handbook of Social Psychology*, 1935, 1051-1093.
[10] B. Malinowski, Culture as a determinant of behavior, in *Factors Determining Behavior*, 1937, 133-168; *Crime and Custom in Savage Society*, 1926, 125.

principle. The vast machinery of culture is maintained and regulated by a body of traditional lore. This is made possible by language which enables man to formulate general rules and condense them into concepts. Thus to systems of action there correspond systems of knowledge which exist even among the lowest primitives. Knowledge, magic and religion are answers of primitive man to the fundamental realities of life. Necessary to remove internal conflict and to organize the community, they become essential factors of spiritual and social integration. Benedict[11] and Mead[12] have made a deep impression upon current thought in social and abnormal psychology by their dramatic portrayals of the culture patterns of primitive groups whose social behavior they have studied. These writers go somewhat further in finding a psychological rather than a biological or economic explanation of the organization of primitive cultures. Culture patterns, they hold, by virtue of their definite configurative sets, act selectively and creatively in accepting, rejecting and re-modelling personality traits, i.e. related sets of attitudes which have individual consistency.[13] Hence it follows that with each culture-pattern there goes in the primitive community an approved type of average or ideal man. Toward the development of this type, the educational systems and all the prevailing institutional attitudes are adapted. Deviants from this type are regarded as abnormal, and are treated as such, abnormality having no meaning except in terms of the approved and authoritative personality-pattern.

When we consider the total import of these recent contributions to psychological theory which stress the importance of the principles of functional interdependence and organic interrelation in every department of social behavior, one thing at least is clear. This is that any attempt to explain such complex products of social life and activity as systems of traditional belief and ceremonial observance, industrial methods, political procedures and artistic creations, in terms of stimulus-response interaction in which stimuli and responses are linked causally as serial events in nature, is foredoomed to failure—the failure that awaits upon all such attempts at over-simplification in the interests of scientific explanation. The result of inter-activity between organism and environment is, in the case of the human individual, to constitute and define for him relational wholes in the way of perceived and imagined objects, purposively effective actions, and emotional-aesthetic experiences. These objects, because they are *not* supplied as identical imprints of the same physical stimulus-pattern to all individuals in the range of its influence, require to be communicated if they are to compose a common social world. As we know, this is accomplished through the associating, by the mechanisms of social conditioning, of these objects, procedures and experiences with a variety

[11] Ruth Benedict, *Patterns of Culture*, 1934, 1-291.
[12] Margaret Mead, *Sex and Temperament in Three Primitive Societies*, 1935, 1-335.
[13] Margaret Mead, The use of primitive material in the study of personality, in *Char. & Personal.*, 3, 1934, 10, 12.

of minor motor responses, chiefly verbal, but also in the way of tentative indicatory movements and changes of facial expression and gesture. Because socially conventionalized and acquired in infancy by all group-members, such responses—particularly, of course, language, which is elaborated *pari passu* with the expansion of the social world—act as stimuli capable of re-presenting (through nervous mechanisms not well understood) both to the individual who makes the response and to the one who hears or sees it, the objects with which it is associated. Thus they serve as means of communication, and upon such communication the development of a social culture is entirely dependent.

More may be learned, however, from an examination of the products of social culture than that they presuppose the intercommunication of the varied results in the way of response-objects which follow in the case of different individuals from the processes of dynamic re-adjustment continually going on between organism and environment. It is reasonable to expect that an examination of the distinctive characteristics exhibited by these products of group culture will throw light upon the organic activities instrumental in their production. Failure to make use of this essential source of information lessens the value of Mead's able analysis of the psychological factors involved in human social life and coöperative activity.[14] This failure is the more striking in Mead's case because of his initial and convincing argument to the effect that all association of the specifically human sort depends on communication which is "responsible for the appearance of a whole set of new objects in nature," and through which alone a living organism attains social selfhood.[15] By keen and penetrating analysis in terms of organic behavior, Mead derives the language-symbol from the animal-gesture and shows the biological function of the latter. But when we ask what it is which, when communicated, gives distinctive character to genuinely social behavior, his answer is 'attitudes,' organizations of different parts of the nervous system that are going to be instrumental in producing acts, and which therefore are capable of representing not only what is immediately taking place but what is going to take place. In such an attitude, the act as a whole can exist in the nervous system determining its later stages, by it can be represented alternative courses of action, and responses to classes and kinds of objects, *e.g.* the characters we assign to 'horse' as idea or concept.[16]

Now it is undoubtedly true that attitudes are thus communicated. But

[14] G. H. Mead, *Man, Self and Society*, 1934, 1-401.
[15] *Ibid.*, 79, 172 f.
[16] *Ibid.*, 11.

just as certainly, objects of perception and thought, and plans of action in their characteristic quality-patterns and spatial, temporal and means-end relations, can be communicated. The equivalent of these objects can be formulated, if desired, in terms (partly hypothetical) of nervous organization, but not without the loss of that structure of differentiated quality and relational pattern which is a key to their vital and social significance. The primitive tribesman who boasts to his fellow of his success in inventing some new weapon or device for killing or capturing an animal does communicate something of his own attitude of self-confidence and self-congratulation, but if his communication is to have much social interest it must reveal something of the nature of the new weapon or plan of attack, and of how it works. Or if it is a magical spell whose evil effects on himself he is lamenting, the content of his communication cannot be exhaustively described for purposes of social psychology in terms of his attitude of hostility or of resignation, but must include some facts concerning the character and operation of the supernatural agency of injury and death.

To the scientific student of the cultural institutions of man, who is desirous of learning what psychological activities engage in their production, it is bound to seem an abortive and illogical procedure directly to convert these culture-products into attitudes and habits of social adjustment. The straightforward and reasonable method, if one seeks such psychological explanation is to begin with an examination of these cultural objects themselves, and to hope to find in their own characteristics the needed clue to the psychological processes and activities operative in their production. When we confront the endless variety of social culture-products, of economic, familial, political and educational institutions, of traditional beliefs, of magic and religion and recreation, this seems indeed a formidable undertaking. But concerning even this vast and diversified array, it is possible to make a preliminary generalization—a generalization which on first mention does not seem to go far beneath the surface, but will furnish at least a starting-point. This is—not, to be sure, that all these varied culture-products are neatly divisible into a few groups—but that they do exhibit three distinct modalities, practical, cognitive, and aesthetic. The first is clearly predominant in industrial methods and forms of political organization, the second in the body of traditional lore, and the third in the various forms of fine art. Other culture-products, such as kinship-systems and family-organizations, religion, magic, education and recreation, show no one of these aspects to be outstanding, but all three

present and inter-penetrating. And no social institution or culture-product (this is a fact to be emphasized) is exclusively practical, cognitive or esthetic; even those most distinctively characterized as one or the other, such as systems of ideas or beliefs, or fine-art creations, possess in some degree the other two modalities in some degree, and all three intimately united.

The presence of these three generic features in varying degree in all forms of social culture points quite plainly and unmistakably to three activities of the human organism as operative in their genesis. In terms of organic behavior, these activities are known to psychologists as verbal, kinetic or motor, and emotional or visceral responses, and so far as they signify the main sorts of adaptive habit, are recognized to be the three vehicles of social behavior. Such descriptive labels are useful in indicating the classes of response-organs exercised by the activity in question. They are too general for purposes of this discussion, however; since they suggest nothing concerning the social function of these activities and, unless this is taken into consideration, it is impossible to be sufficiently definite and apposite in pointing out the specific organic structures which they exercise. If we call them the 'capacities' of articulate speech, of manual-motor contrivance, and of emotional aesthetic expression, this is with no thought of reifying them as faculties, but of connecting specific organic responses with well-defined social functions.[17]

In the case of the first, articulate speech, the connection between definite anatomical structures and the social function of influencing the behavior of others through communication is evident and well-understood. Responses of the specialized bodily organs of vocal utterance, supplemented by gesture and eventually by handwriting, develop through exercise and social learning into language-habits. The social stereotyping of speech-responses and their integration into the elaborate symbolism of language is, as we well know, one of the principal bio-social sources of social culture. The second of the three capacities under consideration stands in need of more detailed and precise description, however. To call it motor or kinetic is quite inadequate to its function in the development of social culture. It is the activity of practical contrivance employed in the fashioning and use of tools, the manufacture of weapons and implements, clothing and habitations, and (with the assistance of speech) in the marshalling of individuals for the preservation of order and discipline, and for group-defense, attack and migration. Primarily of manual, manipulative character, it may be looked upon with respect to organic source as a response of the

[17] L. B. Murphy, *Social Behavior and Child Personality*, 1937, 321.

human hand (with opposed thumb peculiar, as Darwin emphasised, to the human species), assisted by connected muscles of the wrist and arm, and made effective by the erect posture which frees the hand for manipulation and brings it under easy guidance by the eye, and by the mechanisms of bodily locomotion which move it to places of most advantageous application. Likewise the third capacity is insufficiently described for our purposes as visceral. While emotions may originate as responses of visceral muscles and glands, it is not as a visceral response but rather by virtue of its overt bodily expression that emotional activity influences social life and culture. In fact, no more definitely descriptive name suggests itself for this activity than emotional expression, or perhaps emotional-aesthetic expression. This includes responses of the facial muscles in what we call the expression of the countenance, and also the movements of gesticulation, posturing and exclamatory vocalization. The word 'aesthetic' was added because these emotional expressions soon become attached by social conditioning to their observed appearance in the perceptual patterns presented by others' behavior. Hence these expressive movements become not only means of relieving organic tensions but of communicating them to others. It is to be further noticed in this connection that emotionally expressive responses, such as the movements of gesticulation, posturing and vocal exclamation, pass over by degrees into those of dancing, dramatic and pictorial portrayal, and song, whence they develop in due course into the various branches of fine art.

Let us turn back now to the varied forms of social culture and see whether their intrinsic properties and relations do not throw further and very important light upon the organic processes instrumental to their production. Perhaps the most obvious relationship embodied in and displayed by culture-products, a relationship particularly prominent in the wide field of economic institutions, is the means-end relation. The relation of implement or technical procedure to the result or end it accomplishes is the outstanding feature of industrial tools and methods, of weapons and tactics of warfare, and of many political procedures and the ritual performances of magic and religion. Malinowski remarks[18] that he received his first intimation of the character, power and influence of Malenesian magic when he heard the loud chant of the Trobriand magician in the midst of the terrific uproar and commotion of a tropical hurricane. He then realized that while the professed purpose of the magician was to quiet

[18] B. Malinowski, Culture as a determinant of behavior, in *Factors Determining Human Behavior*, 1937, 157, 158.

the storm, the real psychological function of his performance was to restore confidence and optimism among the people. In its simplest form, as illustrated by tools, weapons, and practical methods, this may be called the means-end relation. As exhibited by more complex and highly developed culture-products, such as machines and technical methods and manufactures, it appears as the relation of joint-efficacy, the relation of diverse materials and forces to the unitary result to whose accomplishment they severally contribute.

The attempt has been made by psychologists committed to mechanistic theory virtually to eliminate this means-end relation as a dynamic factor in social life and activity by explaining all means-end adaptation and tool-learning, even among men, in terms of trial-and-error reaction in the case of the first, and of the establishment of symbolically conditioned habits of motor response in the case of the second. But this attempt may be confidently said to have failed. As has been well stated, in describing the procedure of invention in the field of material culture: "The lucky accident is not enough, it must be seen by a person who has in mind some problem to solve."[19] Experimental studies have proved that insight into instrumental relations frequently governs constructive play and tool-learning with very young children, and, so far as the use of material objects as instruments is concerned, is sometimes operative in the behavior of apes. Relevant to the question are also the conclusions of Tolman[20] and fellow-investigators that, in order to explain the learning of animals much lower in the scale of nervous organization than the ape, we must attribute to the organism of the animal (the maze-running rat) certain 'means-end readinesses' which enable it to respond to environmental objects as stimuli, in their instrumental or tool-serving, relation to the goal.

A second outstanding feature possessed, or relationship exhibited by the products of human social culture, is consistency and coherence of 'meaning,' *i.e.* of logical structure and pattern. This of course applies particularly to the intellectual tradition of the group, the body of ideas and beliefs accepted and handed down by successive generations. But while pertaining chiefly to the ideational field, this relation of logical consistency or pattern extends its influence over many fields of social practice, such as religious ceremonial, family organization, marriage customs, and even (in the prescriptions and taboos it sanctions) over such routine matters as the choice and preparation of food. It is a fact, well-established but still needful of emphasis, that orderly systems of concepts, defined and employed with a

[19] C. Wissler, Material culture, in *Handbook of Social Psychology*, 1935, 560.
[20] E. C. Tolman, *Purposive Behavior in Animals and Men*, 1932, 29, 95.

fair degree of consistency to represent classes and kinds of objects, so far from being exclusively a by-product of advanced culture and intellectual sophistication, play an important part in the social life of some, possibly many, primitive groups. Such an ideological system may provide the theoretical basis for an elaborate division and subdivision of the tribe on a totemistic basis into exogamous groups, or for a complicated method of reckoning relationship and transmitting name and authority, or it may supply the mythological foundation and framework for magical rites and religious ceremonial which stimulate and direct coöperative effort. No doubt some of these primitive ideologies relating to agriculture or animal husbandry have definite economic value, and it cannot be doubted that a body of traditional lore, commonly believed in and understood, would increase mutual understanding and confidence, hence be functionally effective in strengthening the coöperative tendency. But the most striking evidence of the reality and importance of this feature of consistency and coherence of generalized (conceptual) character which man finds in his world when he responds to it intellectually is furnished by the tenacious adherence of primitive groups to customs and practices in their total effect practically disadvantageous, because they follow logically from some traditionally authorized scheme of social relations. Thus according to Margaret Mead[21] the social life of the Mundugomor suffers great injury because of their adherence to the principle or conception of the "rope" in organizing family life, a system which neither organizes people into patrilineal or matrilineal groups, but is based on the theory of a natural hostility that exists among all members of the same sex and the assumption that the only possible ties between members of the same sex are through members of the opposite sex.

The organic activity through which man responds to consistency of generalized character and permanence of pattern in the world, by constructing ideological systems, is that of articulate speech. Speech enables man to respond to the attributes and qualities which identify classes and kinds of objects, and to the patterns of order and relation which these objects present, by giving them names. Thus he obtains for these products of intellectual analysis and abstraction, concrete symbols in the way of internal motor responses capable of unlimited variation, abbreviation and combination among themselves, which are capable (through mechanisms set up by kinaesthetic conditioning) of acting as stimuli to arouse the patterns of brain-activity which represent these significant features of the real world. While experimental results may indicate, as a recent work informs

[21] Margaret Mead, *Sex and Temperament in Three Primitive Societies*, 1935, 176.

us[22] that logical consistency plays a minor part in determining what attitudes or cultural interests are held simultaneously by the individual members of civilized society, this is in large part due to the impact of new and arresting forms of social stimulation against which the intellectual powers of the poorly educated majority are able to make little or no headway. Taking human history as a whole, there is no room for doubt as to the influence of social tradition when reinforced by congruent institutional stimulation, or that, other things being equal, this influence is proportionate to the logical order and consistency of the established system of beliefs.

The products of primitive art used for the embellishment of social, ceremonial and religious ritual, and for the adornment and decoration of implements of industry and weapons of war, clothing, family habitations and communal houses, bring to our notice a third type of relationship. This is the patterning of colors and textures, of forms and movements, of tones and rhythms, into schemes or designs whose order and arrangement afford pleasure both by virtue of their own intrinsic properties and because of the objects of common human interest and concern which they suggest to the imagination. Without attempting an incursion into the field of aesthetics, it will be sufficient for purposes of the present discussion to mention two facts which are, as I think, evident and beyond dispute. One is that beauty in art-products depends upon their possessing in some degree certain objective properties, such as order and organization, symmetry, proportion and balance among their diverse sensory components. The second fact is that these properties, familiar and indisputable, connect themselves with activities of emotional expression in the human organism. It has been already noticed that the various kinds of artistic creation, the dance and song, dramatic portrayal and pictorial representation, are outgrowths of the emotionally expressive responses of gesture, bodily movements and posturings, vocal cries and verbal repetition, accent, etc. By means of these overt responses the individual expresses strong feeling, and, through the perceptual patterns which they arouse in others, communicates to them the emotions which stir him. This aspect of the aesthetic experience has been fully treated by Langfeld in an article wherein he states: "There is little doubt that aesthetic expression is the best means for the communication of affective states and therefore one of the most fruitful sources for the study of such communication as well as for an examination of the states themselves."[23]

[22] G. Murphy and others, *Experimental Social Psychology*, 1937, 1031, 1045.
[23] H. S. Langfeld, The place of aesthetics in social psychology, *Brit. J. Psychol.*, 27, 1936, 141.

We may imagine, as the starting-point of this particular cultural development, an individual in the throes of strong emotion and by its expression furnishing himself with visual, auditory and kinaesthetic stimuli. If, on occasion, the pattern of sense-imagery presented, or the sequence and order of vocal sounds, of own bodily movements seen and internally sensed, was such as to arrest attention by its own satisfying properties, to increase and stabilize pleasurable emotion and to offset or divert painful emotion, he would tend to repeat it with resulting fixation in habit. Such patterns would tend to be imitated through ordinary processes of conditioning, particularly by those affected by the same emotion. If extended, elaborated and refined by technical contrivance, they would become incorporated in the cultural heritage, musical, dramatic, decorative, etc., of which later generations would avail themselves in the expression and communication of emotion. The descriptions of Benedict and Mead have supplied us with impressive and convincing illustrations of the importance of art in giving unity, imparting individuality, to different primitive cultures. We think of the Zuni tribe whose life has centered in the artistry of socio-religious dance and ceremonial, and of the Tschambuli who live principally for art and among whom the women are considerate and good humoured enough to provide the opposite sex with the economic wherewithal in order that each man may be an artist.

When we review thus briefly the outstanding features or (as it has been convenient to call them) relationships exhibited by the main classes of culture-products, it appears that they do throw light upon the nature and bio-social function of the three organic activities responsible for their production. The foregoing paragraphs will have been seriously misleading, however, if they have seemed to imply that each of these three activities of articulate speech, practical contrivance, and emotional expression worked in independence of the other two or was alone operative in any major department of human culture. Of course, just the opposite is true. The three work in closest interdependence and are jointly effective in every field of social achievement. In the work of mechanical contrivance and social invention, men deal in anticipative forecast and methodological routine with classes and kinds of materials and arrangements, forces and processes, verbally symbolized and unceasingly talked over. They are influenced in the credence they give to old beliefs and new conjectures by the practical results achieved when they act as these ideas suggest or direct. The artist must exercise a high degree of technical skill in the manipulation of his materials if he is to obtain the desired aesthetic effect. The coöperative employment of skilled handicraft in the production of masks, effigies and

decorative details to beautify some religious ceremonial has its inspiration in mythological lore of which workers and watchers are continually reminded. Men talk (as well as whistle) while they work; their hands are busy with routine tasks as they discuss affairs of this world and the next. What is affirmed, therefore, is only that each one of these three activities is dominant in a certain major department of social culture because it responds effectively to that feature or relationship of external objects with which this branch of social achievement is primarily concerned.

In view of the evidence thus supplied, we cannot doubt that each one of these three activities of conceptual verbal formulation, practical invention and emotional-aesthetic expression, when persistently and skillfully employed, may become an independent and original source of satisfaction to the human individual. It follows then that culture-products have values of their own, based on the fact that objects of the external world possess properties and disclose relations which answer to the efficacy of these three activities—in addition to the values they derive from the fulfilment of organic and economic need. This would mean that the weaving of the accumulated observations and experiences of generations into a coherent and logically formulated tradition has its own intrinsic interest and value, that the power gained over physical forces and over animal and human behavior-tendencies through the invention of technical instrumentalities is likewise an independent and authentic source of satisfaction, that music and dancing, dramatic and pictorial representation, are delights in themselves apart from any ulterior purpose which they may subserve. There is evidence to show, in the opinion of many psychologists, that any effectively organized adaptive response, regularly exercised and firmly intrenched as habit, acquires a motive force of its own. But the activities under consideration are much more than examples of complex response-integrations stabilized as habits through social training, of which there are many in the field of social behavior. Each of the activities in question exercises a specific set of bodily response-organs, inter-adjusted to serve a single bio-social function, and the combined effect of the operation of all three is to bring into existence a type of social life distinctive of the human species which depends upon the communication of verbal interpretations and emotional experiences, and the coöperative exploitation of the resources of the natural world.

So much may be affirmed, without at all denying that these cultural activities have in the course of social history been mainly employed in the service of organic and utilitarian need. Men were first prompted to recall and talk over past experiences, to make plans and to discuss practical ways and

means, in order to insure their own safety and a continuation of those enjoyments attendant upon the satisfying of primary appetites. They devised implements and weapons, they contrived methods of tilling the soil, of domesticating animals and of constructing houses, in order to obtain food, clothing and shelter. Music, dancing and dramatic performance were engaged in, masks, costumes and decorations were introduced, to enhance the effect of religious ritual which had the social utility of intensifying the feeling of corporate unity and enthusiasm needed for coöperative effort in hunting and warfare. And such intrinsic satisfactions as these cultural activities themselves yielded were only incidental to the successful discharge of the routine duties of industrial and family life. It is only at a late period in social evolution and in culturally advanced societies that knowledge, practical power, technical mastery, and beauty, are sought as intrinsic values, or ends in themselves. But it in nowise follows from these undeniable facts that distinctive cultural interests do not exist or that they have not been in some measure operative as motivating factors throughout the whole history and development of man's social culture. Consequently, it is impossible to agree with a view held by not a few social psychologists that in all the higher intellectual and artistic vocations the autonomic drives of hunger and sex are the fundamental motivating force.[24] Or with the assertion that the goal-functions of culture are the satisfaction of the four wishes for security, response, recognition and new experience.[25]

Field-theoretical explanation, as formulated in the works cited from Lewin and Brown, has special advantages when applied to the problem of the part played by values in social behavior. Field-theory understands the behavior of human individuals in terms of the structure of the organism-environment field as constituted by the dynamic interplay of physical and social factors. Such an approach is illuminating in explaining the function of values, including the values of social culture-products with which we are concerned. First, because it makes values objective, converting them into 'field' forces. No longer sequestered in the consciousness of the subject, they operate as positive and negative valences, the attracting forces of goal-objects and the repelling forces of barriers or obstacles to goals. And not only are values themselves objectively operative; they determine the dynamic organization of the whole field, *i.e.* the parts played by various objects in it. In such a field, certain objects derive position, prominence and meaning from their relation as ways or means to the goal; other objects derive character and importance from their relation as obstacles or barriers

[24] See *e.g.* F. H. Allport, *Social Psychology*, 1924, 65.
[25] T. K. Folsom, *Social Psychology*, 1931, 493.

to the goal. Furthermore—and this is the vital point in the present context—not only does value determine the objective organization of the behavior-field for the agent projecting a course of action; it does so also for other individuals in the field who observe his behavior. They observe his behavior (to repeat the term recommended by F. H. Allport) *teleonomically*. They perceive his actions in their transitional, or functional, relation to the result required to round out and complete them, in their 'gap-closing' function. So understood, values take their rightful place as objective factors operative and observable in the world of common perception and action.

Field-theory has a second advantage of enabling us to explain how a group of individuals can participate in the realization of a common set or system of values such as those of social culture, without having recourse to a hypothetical social consciousness of an inclusive sort in which all share. Such a participation on an objective basis becomes a simple empirical fact, so far as we directly perceive (not infer) what others are trying to do; the aimfulness of their performance. Brown refers in illustration to the way in which, in the economic class-struggle, the divisions between classes constitute barriers which possess common negative valence for all members of the inferior class.[26] To understand the bearing of this upon participation in the values of social culture, it is essential to bear in mind that the crucial fact here on which the whole argument depends, *viz.* that we perceive others' behavior in functional relation to the result which completes it, applies not only to the observation of outwardly directed action or motor performance, but also to perceiving what they say and how they feel. When talking with an acquaintance I do not, except in rare cases, first listen to and perceive his words, then translate these verbal symbols into terms of my own conscious experience and finally reach a conclusion as to their meaning. Instead, I respond to the sounds of his voice by perceiving or knowing the objects or relations to which they refer. The words may be few, grammatically disjointed and fragmentary, but sufficient to evoke in me the dynamic brain-patterns which present to me the objects his thought is concerned with. So also with the expressive movements of others; we perceive them dynamically, in terms of the instigating objects toward which the organic and personal tension displayed in his behavior is directed.

The type of explanation to which reference is made has one limitation, however, when utilized to account for the influence of the values of an organized

[26] *Op. cit.*, 137.

social culture upon individual behavior. The principles of field-theory as formulated by its authors have been developed, at least in the social field, almost exclusively in connection with the processes of social adjustment through overt action, while the values of social culture are dynamic factors in the cognitive and aesthetic fields as well. Thus Brown, who has dealt most extensively with the social applications of field theory, characterizes the activity of individuals in the social fields as social locomotion. He makes it quite clear, to be sure, that by locomotion he does not mean physical movement but rather the sequence of changes by which the individual reaches his goal, and by freedom of social locomotion the number and diversity of goals open to attainment by the individual in the social world.[27] But the social activities with which he is almost entirely concerned in his detailed exposition are those of inter-group adjustment where goals are attainable only through overt action and motor performance. The limitation in question is perhaps more clearly apparent in the distinction made by Lewin and Brown between different degrees, planes or dimensions of reality in the psychological activities of the individual. According to Brown the plane of fundamental reality is that of perceiving and work; other planes of less reality are (in order of decreasing reality) conceptual thought, phantasy and dreaming.[28] It is true that in an earlier context he makes the reservation that thinking, or even day-dreaming, may in certain circumstances have a higher degree of reality than perceiving. But so far as social behavior is concerned, it is plain that the primary function assigned to conceptual thought and imagination is that of substitute or compensation for the more real activity of overt social adjustment. The plane of fundamental reality in social life is that of perception and work. Now most assuredly the compensatory function of 'rationalization' and day-dreaming in social behavior is highly important. But to neglect the part played by conceptual thought and constructive imagination in ordering and stabilizing social relations on more real and satisfying levels is seriously misleading. One thinks of the advance from the earth-centered and man-centered view of the world (which remains that of common perception) to the conceptual cosmic system of modern science which is the basis of modern civilization.

The truth is that conceptual thought, so far from removing those who exercise it from the actual world of social life and intercourse, is prerequisite to coöperation among human individuals in any real sense of the world. This is the view confirmed by Piaget's investigations, which have shown that intellectual development from the ego-centrism and animism of early childhood to the objective and mechanistic view of the later period, proceeds concurrently with increasing social coördination which culminates in the capacity for genuine coöperation. For voluntary coöperation among equals (which as Piaget emphasises is the foundation of democracy in social and political organization)[29] requires that the individuals engaged, while preserving severally their own perceptual perspectives and unique interests and aptitudes, shall have, as a common field of action, a world of objects whose relations are defined and ordered similarly for all. It could not be otherwise, since only thus could a group of freely acting individuals pool their original efforts on the common working basis required for productive achievement under the conditions set by the determinate sequence of events in the external world. Analogous factors shape

[27] J. F. Brown, *op. cit.*, 51, 58.
[28] *Op. cit.*, 285.
[29] J. Piaget, *The Moral Judgment of the Child*, 1932, 324-325, 366.

the social situation in the field of discussion. Exchange of ideas and information, of opinion and belief, through the symbolism of speech, would not be interesting or fruitful except for individual differences in outlook, experience and interpretation, which make such communications worth while and create whatever flavor and piquancy they possess. But equally would such communications be totally devoid of interest, would be quite unintelligible and meaningless, if they did not all refer to a common objective world, and have their setting in a common framework of objective relations whose logical coherence and pattern are the work of conceptual thought. In the case of phantasy, there is more ground for holding that it possesses less reality as a field of socio-psychological activity than does the world of perception and overt performance. But here a distinction must be made. If we refer to such flights of subjective phantasy as are found in idle reverie or 'castle building' or in the compensatory day-dream, we deal with a field of activity admittedly lacking in objectivity, whose objects have little general social significance. But if it is the phantasy of the creative artist we have in mind, the situation is quite different. Few would deny that the creations of high-grade artistic imagination possess reality in their way as great as observed facts of nature and social behavior, or even scientific theories; would deny, in short, that the field of aesthetic appreciation and communication possesses social objectivity. This is not primarily because art-products embody sensuous properties and patterns (though this is essential), or that they conform to the conceptually formulated order of object and events (though this is necessary, at least to some slight degree), but principally because of the effect they have in arousing and releasing, steadying and tranquillizing, emotion. By virtue of these properties, objects which arise as flights of individual phantasy have the power of suggesting features of vital and enduring significance in the life-history of man and the nature of the world. Hence their production and enjoyment constitutes an objective field of social culture in whose activities and satisfactions all individuals can to some extent participate.

The social psychologist may therefore agree that some of the values which men jointly realize in producing the works of social culture are objective and universal, without assigning these values to a transcendental or sublimated sphere. The values of knowledge, of practical power or technical mastery, and of beauty are objective because based on properties and relationships of the environing world. They are realized by three activities of the human organism, the bio-social activities of articulate speech, practical contrivance and emotional-aesthetic expression, which answer to these properties and relationships of external objects and events. The values in question are universal because realized to some extent by all who employ the three activities in the daily routine of social adjustment. Their realization, at the start and in the main, is incidental to the fulfilment of organic and economic need. Only by exceptional individuals, whether in primitive groups or in civilized societies are they pursued as goals for their own sake, but these few make a disproportionately large contribution to the sum-total of man's cultural achievements.

'SUBJECTIVE' COLORS FROM LINE-PATTERNS

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The color-effects observed when one gazes steadily at a black and white pattern, *e.g.* a tiled floor or a piece of finely cross-ruled paper, have recently been mentioned in two separate reports. The first report, made by Skinner in 1932,[1] describes "a paradoxical color effect" from an arrangement of white disks on a black background (see Fig. 2 j); and the second, by Luckiesh and Moss in 1933,[2] reports "definite color-effects" from parallel diagonal lines (see Fig. 3). Skinner thought that his paradox was something new, but Luckiesh and Moss related the color-effects of their pattern to those which Benham produced with disks in 1894.[3]

The phenomenon reported by Skinner and by Luckiesh and Moss is an old one; at best it is a case of old wine in new bottles. Counting the present instance, it has been 'discovered' no less than five times. It was first observed and reported by Fechner in 1838.[4] He discovered it accidentally while working on a black and white disk. He had prepared a disk of 18 concentric rings which varied, by equal amounts of black and white, from the innermost that was all black to the outermost ring that was all white (see Fig. 1 a). Expecting to see a series of gray rings differing in shade when the disk was rotated, he was astonished to discover that colors appeared at rotations below the rate of fusion. He found that different colors were reported by different *O*s, that the colors ranged from red to greenish blue, that they were usually not highly saturated, and that their saturation and their position on the disk depended upon the speed and direction of rotation.

Fechner's observations were verified by a number of his contemporaries[5] (see

* Accepted for publication June 1, 1935.

[1] B. F. Skinner, A paradoxical color effect, *J. Gen. Psychol.*, 7, 1932, 481-482.

[2] M. Luckiesh and F. K. Moss, A demonstrational test of vision, this JOURNAL, 45, 1933, 137 f.

[3] Anon., An "Artificial spectrum top," *Nature*, 51, 1894, 113 f.

[4] G. T. Fechner, Ueber eine Scheibe zur Erzeugung subjectiver Farben, *Pogg. Ann. d. Physik u. Chemie*, 121, 1838, 227-232.

[5] H. W. Dove, Ueber Scheiben zur Darstellung subjectiver Farben, *Pogg. Ann. d. Physik u. Chemie*, 151, 1848, 526-528; *Farbenlehre*, 1853, 281-283.

Ernst Brücke, Untersuchungen über subjectiver Farben, *Pogg, Ann. d. Physik u. Chemie*, 160, 1851, 418-447; Ueber den Nutzeffect intermittirender Netzhautreizungen, *Ber. d. kaiser. Akad. d. Wiss., math.-natur. Kl.*, 49, 1864, 128-153.

Ogden Rood, On a new theory of light, proposed by John Smith, *Amer. J. Science & Arts*, 30, 1860, 182-186; Subjective optical phenomena, *ibid.*, 31, 1860, 417.

David Brewster, On certain affection of the retina, *L., E., & D., Philos. Mag. & J. Science*, 4th ser., 21, 1861, 20-24.

H. Aubert, *Physiologie der Netzhaut*, 1865, 377-380.

H. Helmholtz, *Handbuch der physiologischen Optik*, 1, 1867, 380.

Sigmund Exner, Bemerkungen über intermittirende Netzhautreizung, *Arch. f. d. ges. Physiol.*, 3, 1870, 214-240, esp. 230-234.

A. Kunkel, Ueber die Abhängigkeit der Farbenempfindung von der Zeit, *Arch. f. d. ges. Physiol.* 9, 1874, 197-220.

Fig. 1 b–i) and various explanations of the phenomenon were offered. Fechner himself explained the colors, which he called "subjective," by retinal inertia. The effect of white light upon the retina did not, so he thought, die out with equal rapidity for all the rays. Helmholtz, on the other hand, was of the opinion that the 'subjective colors,' which were dubbed "Fechner's colors" by Brücke in honor of their discoverer,[6] were due to differences in the latent periods of the various hues, *i.e.* to the differences in time at which the different colors reached their maximum intensity.

While the explanation of these colors was being discussed in Germany, John Smith in England, in 1859, 'discovered' for a second time that colors could be produced "by alternate light and shade in various proportions."[7] He used a disk (see Fig. 1 d) upon which five concentric rings of various proportions of black and white were painted, and obtained a "great variety of colors . . . amongst others, a pure red, and various shades of purple, pink, yellow and blue." The same results, he found, could be produced "by cutting out spaces in the white card and causing it to revolve on a black surface."[8] He "was satisfied that the experiments were original and not explicable by the present recognized laws." He thought that he had demonstrated "that varieties of color are produced by pulsations of light and intervals of shadow in definite proportions for each shade of color."

A report of Smith's experiments published in America[9] led Rood to repeat the observations.[10] Though corroborating Smith's results, Rood disagreed with his explanation. He preferred to take his stand with Fechner, who had, 20 years earlier, as he pointed out, come to the conclusion that the colors were subjective.

Interest in the phenomenon died out and it was apparently forgotten. In any case, the third 'discovery' was made by F. J. Smith in England in 1881.[11] He discovered accidentally, while revolving the large wheel of a bicycle between himself and the source of light, that various colors were produced by the interruptions of the spokes. "Different rates of revolution give different colors," he reports. When, for example "120 spokes pass before the observer per second, violet light shines out vividly; when 65 pass, red appears." He thought that the interruptions of the spokes decomposed the light and that there was a relationship between spoke-interruptions and wave-length. Soon after the publication of this report, Hannay,[12] using black-and-white disks which were rotated 'too rapidly for the eyes to notice the pattern and too slowly to combine into a gray,' explained the color effects that

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[6] Cf. E. C. Sanford, A laboratory course in physiological psychology (Exp. 137, Some phenomena of rotating disks: Fechner's colors), this JOURNAL, 5, 1893, 401; also F. W. Bagley, An investigation of Fechner's colors, this JOURNAL, 13, 1902, 488 f.

[7] John Smith, On the production of colour and the theory of light, *Rep. Brit. Ass. Adv. Science*, 29, 1859, 22-23.

[8] A method recently utilized by G. A. Fry, see footnote 34 below.

[9] John Smith, On the cause of color and the theory of light, *Amer. J. Science & Arts*, 29, 1860, 276-278.

[10] Ogden Rood, On a new theory of light, proposed by John Smith, *Amer. J. Science & Arts*, 30, 1860, 182-186; Subjective optical phenomena, *ibid.*, 31, 1860, 417.

[11] F. J. Smith, Apparent decomposition of sunlight by intermittent reflecting surfaces, *Nature*, 24, 1881, 140.

[12] J. B. Hannay, Colour perception, *ibid.*, 25, 1882, 604-605.

he obtained in terms of the latent periods of the different color-organs. The eye responded, as he believed, quickest to red, a little more slowly to green, and slowest

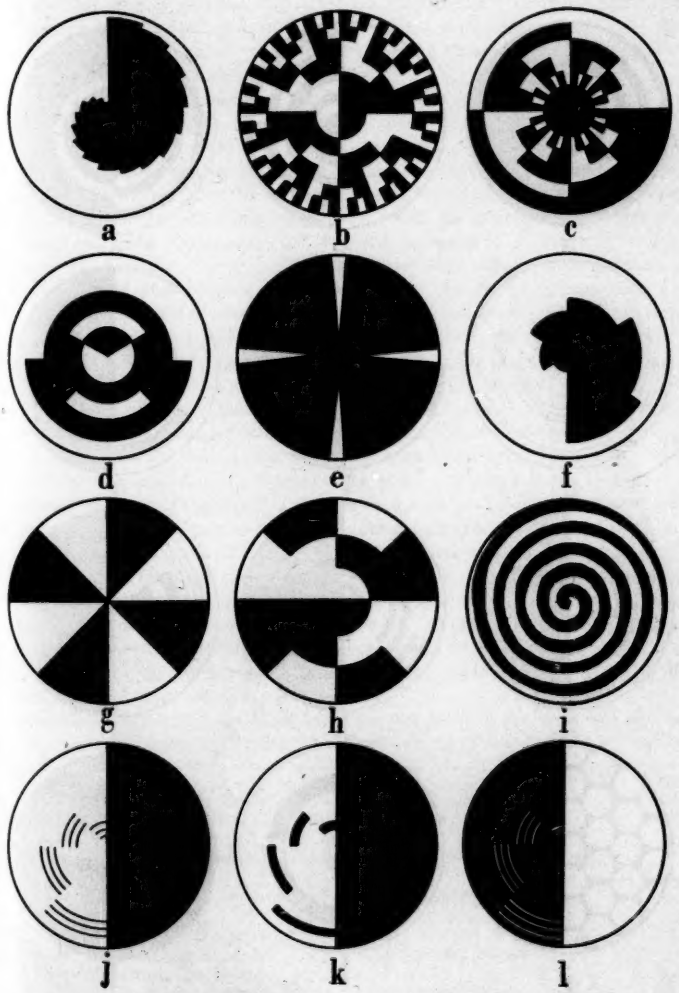

FIG. 1. VARIOUS DISKS USED IN THE PRODUCTION OF 'SUBJECTIVE' COLORS

a, Fechner; b and c, Brücke; d, John Smith; e, Rood; f, Aubert; g, h, and i, Helmholtz; j, Benham; k and l, Finnegan and Moore.

to blue. He thought, as did John Smith before him, that the experiment was of great theoretical importance as its explanation would "allow of a decision being arrived at between the theories of Young and Hering."

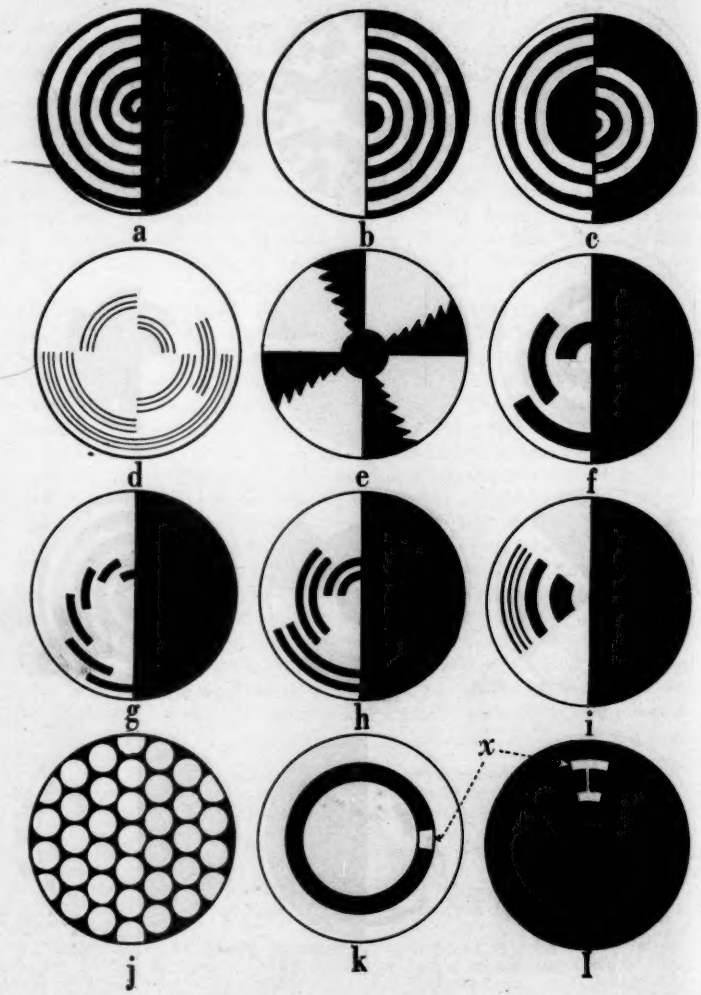

FIG. 2. VARIOUS DISKS USED IN THE PRODUCTION OF 'SUBJECTIVE' COLORS

a, b, and c, Hurst; d, Bagley; e, f, g, and h, Baumann; i, Piéron; j, Skinner (stationary); k and l, Fry (*x* represents cut-outs).

Napier Smith[13] objected to Hannay's explanation, arguing that a given speed of rotation should, according to Hannay, yield but one color, while as a matter of fact one may observe "a dozen different colors side by side." Though venturing no explanation himself, Napier Smith pointed out that explanations which state that the effects are "physiological" and that the colors are "subjective" are not explanations in the true sense of the word.

Sanford included an experiment entitled "Fechner's colors," with citations from Helmholtz and Aubert, in the "Laboratory course in physiological psychology" which he published in 1893.[14] Despite that reference, the phenomenon was 'discovered' for a fourth time in 1894, when Benham constructed his "artificial spectrum top."[15] The top, a toy rotated between thumb and finger, carried a black and white disk of novel construction. "One half is black, while the other half has twelve arcs of concentric circles drawn upon it." The arcs, arranged in 4 groups of 3 each, were of 45° (see Fig. 1 j). Possibly because these disks differed from the old and also because the color-effects were greatly enhanced, the old phenomena were not recognized and the investigations which followed were reported without reference to the earlier work as if they concerned something entirely new. Numerous investigators studied the phenomena under various conditions. For example, Liveing varied the speed of rotation[16] and substituted sodium light for daylight.[17] Benham, too, used sodium light[18] and varied the speed of rotation.[19] Abney used monochromatic and mixed lights.[20] Finnegan and Moore varied the structure of the disk—making the arcs 1 cm. broad instead of lines, and placing white linear arcs on the black half of the disk instead of black arcs on the white (see Fig. 1 k and l)—and the speed and direction of rotation.[21] Turner devised a lantern slide of Benham's disk which enabled him not only to demonstrate the phenomenon to large audiences but also to study it by means of transmitted light of varying color and composition.[22] Hurst developed three new disks—"in which one half the disc was black and the other white, with a white spiral on the black, or a black spiral on the white, or with both at different distances from the centre on the same top"[23] (see Fig. 2 a, b, and c)—which intensified the phenomenon and which could be used without infringing upon the copyright on Benham's disk.[24] Bidwell varied intensity of illumination and speed and direction of rotation.[25]

Regarding the observations there was little dispute. The hue, tint, and chroma of the 'subjective' colors were found to depend upon speed and direction of rotation

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[13] Napier Smith, Colour perception, *ibid.*, 26, 1883, 30-31.
[14] *Op. cit.*, this JOURNAL, 5, 1893, 401 (Experiment 137).
[15] Anon., *op. cit.*, *Nature*, 51, 1894, 113 f.
[16] G. D. Liveing, On Benham's artificial spectrum, *ibid.*, 51, 1894, 167.
[17] *Ibid.*, 200.
[18] Benham, The artificial spectrum top, *Nature*, 51, 1894, 200.
[19] *Ibid.*, 321.
[20] W. de W. Abney, The artificial spectrum top, *Nature*, 51, 1894, 292.
[21] J. M. Finnegan and B. Moore, The artificial spectrum top, *ibid.*, 292.
[22] Dawson Turner, The spectrum top, *ibid.*, 439.
[23] C. H. Hurst, A true spectrum top and a complementary one, *ibid.*, 510.
[24] Newton and Co., The artificial spectrum top, *ibid.*, 463.
[25] Shelford Bidwell, On subjective colour phenomena attending sudden changes of illumination, *Proc. Roy. Soc. London*, 60, 1896, 368-377; On the negative after-images following brief retinal excitation, *ibid.*, 61, 1897, 268-271.

and upon intensity, hue, and complexity of illumination. With a change in direction of rotation, the order of the colors on the disk is reversed. With very rapid and very slow rotation no color results. With extremes of illumination the saturation of the colors is reduced.

Regarding the explanation of the observations there was, on the other hand, small agreement. Even after Edridge-Green,[26] and Cattell[27] in 1895, called attention to the fact that Helmholtz, Rood, and Fechner had years before experimented upon and explained the phenomenon, the disagreement continued. The majority opinion, as shown in Bowditch's review in 1896[28] and Rivers' in 1900,[29] leaned, however, toward the theory of Helmholtz that the colors were due to differences in latent period, *i.e.* to the fact that red rises more quickly than green and that green rises more quickly than blue—facts attested to by Kunkel.[30]

In 1902, Bagley,[31] after a review of the literature, undertook the most comprehensive and thorough study of the phenomenon since Brücke. She studied the effects of rate of rotation, width and length of line, relative size of sectors, contrast, intensity of illumination, and background (see Fig. 2 d). Since that paper, the phenomenon has been extensively studied by three investigators: by Baumann,[32] from 1912-1918, who noted the effects of illumination, size of disk, order and size of sectors, and *O*'s distance from the disk (see Fig. 2 e-h); by Piéron,[33] in 1922, who used several different disks (see Fig. 2 i) and monochromatic illumination; and by Fry,[34] in 1933-1934, who, using transmitted light and disks with open sectors (as John Smith, Rood, and Hurst did before him), observed the effect of the following variables: brightness of the open sectors; frequency and duration of exposure (*i.e.* rate of rotation and size of sector); and brightness and frequency of stimulation of the surrounding retinal areas (see Fig. 2 k and l).

In the recent reports[35]—the fifth 'discovery'—the phenomena were obtained from stationary patterns. Heretofore, rotating disk-patterns had always been used. Indeed, according to Bagley, movement is "one of the essential conditions upon which the production of the colors depends."[36] Movement may, however, be involved even

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[26] F. W. Edridge-Green, The artificial spectrum top, *Nature*, 51, 1895, 321.

[27] J. McK. Cattell, The spectrum top, *Science*, 2, 1895, 13.

[28] H. P. Bowditch, The sense of vision, in W. H. Howell's *An American Textbook of Physiology*, 1896, 789 f.

[29] W. H. R. Rivers, Vision, in E. A. Schäfer's *Textbook of Physiology*, 1900, 2074 f.

[30] A. Kunkel, Ueber die Abhängigkeit der Farbenempfindung von der Zeit, *Arch. f. d. ges. Physiol.*, 9, 1874, 197-220.

[31] Bagley, *op. cit.*, this JOURNAL, 13, 1902, 488-525.

[32] C. Baumann, Beiträge zur Physiologie des Sehens: IV. Subjektive Farbenerscheinungen, *Arch. f. d. ges Physiol.*, 146, 1912, 543-552; V., *ibid.*, 166, 1916, 212-216; VII., *ibid.*, 171, 1918, 496-499.

[33] Henri Piéron, Le mécanisme d'apparition des couleurs subjectives de Fechner-Benham, *Année psychol.*, 23, 1922, 1-49.

[34] G. A. Fry, Modulation of the optic nerve-current as a basis for color-vision, this JOURNAL, 45, 1933, 488-492; Color phenomena from adjacent retinal areas for different temporal patterns of intermittent white light, *ibid.*, 714-721; Color sensations produced by intermittent white light and the three-component theory of color-vision, *ibid.*, 46, 1934, 464-469.

[35] Skinner, *op. cit.*; Luckiesh and Moss, *op. cit.*

[36] Bagley, *op. cit.*, 489.

in the stationary patterns, since the colors may result, as Luckiesh and Moss suggest, from "rapid eye-movements" over them and thus the "effect may be analogous to the color-effects produced by a rotating Benham disk."[37]

Problem. Our problem in this study springs from the suggestion that the color-effects are due to eye movement. We wished specifically to determine what part, if any, eye movement played in the perception of these

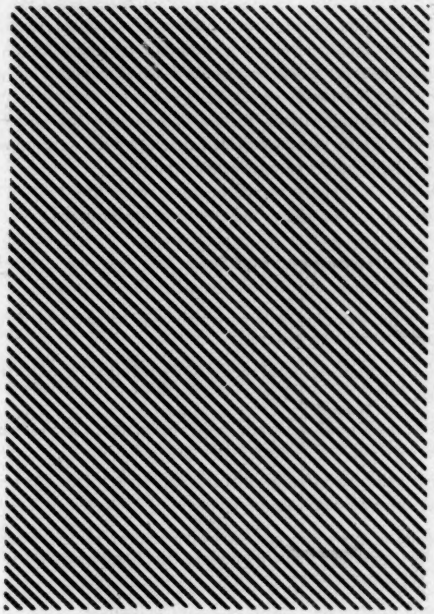

FIG. 3. THE LUCKIESH-MOSS STIMULUS-PATTERN
(Actual size used; centered on 6 x 9 in. sheet.)

'subjective' colors. Before turning to that problem, however, we undertook to learn the effect of (I) knowledge and instruction, (II) color of pattern and background, and (III) width of line and spacing. We wanted to discover what conditions yielded the most pronounced effects so that we could use them in the main experiments (IV) on eye movement.

In all parts of the study, we used the Luckiesh-Moss pattern (Fig. 3) with occasional modifications which will be described in the proper context.

[37] Luckiesh and Moss, *op. cit.*, 137 f.

I. Effect of Instruction and Knowledge

The first part of our study concerned the effect of instruction. Owing to to the recent publication of the stimulus-figure[38] and to our students' familiarity with the phenomenon, we were unable to obtain a group of competent *O*s who were unacquainted with the color-effects. We therefore added knowledge of the phenomenon to our list of experimental variables.

Procedure. The stimulus-figure was a pattern of diagonal lines about 0.5 mm. wide and about 1 mm. apart, covering a rectangle approximately 9 x 6.5 cm. (see Fig. 3) and printed in dead black on egg-shell white paper. This pattern was presented to *O*, for 1 min. by means of a simple exposure apparatus, in a neutral gray booth illuminated by daylight mazda bulbs. *O*'s head was held firmly in position by means of a head rest which was so adjusted that *O*'s eyes were opposite, and 14 in. from, the center of the stimulus-pattern when exposed.

Observers. Of the 22 *O*'s who served in this part of the study, 9 were acquainted with the phenomenon under study and 13 were uninformed. All were competent observers.

Instructions. Three instructions were used. The first was general, simply asking the *O*s to observe the figure and to describe what they perceived.

(a) "Look at the stimulus-pattern, which will be exposed directly in front of you, and describe what you observe."

The second instruction was also general regarding *O*'s task, but specific as to his visual fixation.

(b) "Look at the stimulus-pattern with far-fixation, *i.e.* fixate beyond the plane of the pattern, and describe what you observe."

The third instruction specifically told the *O*s what to look for and what special phenomenon they were to describe.

(c) "Concentrate upon the pastels which appear in and among the diagonals and describe the qualitative changes (hue, tint, and chroma) that occur during the course of the observation."

Every *O* served three times under each instruction.

Results. (1) 'Subjective' colors. Table I shows the distribution of the *O*s, observing with and without knowledge, who reported colors in at least one of the trials under the three instructions.

Knowledge of the phenomenon favors, but by no means guarantees, observation and report. Two of our 9 *O*s serving with knowledge did not report, and as developed in answer to questions after the experiment, did not observe the colors under instruction *a* or *b*. Neither, on the other hand, does a lack of knowledge guarantee that the colors will not be seen. Approximately a third of the *O*s serving without knowledge, 4 out of 13,

[38] Our study was begun during the spring of 1933, a few months after the publication of Luckiesh and Moss's observations.

reported colors under *a;* and under *b* the number was increased to about half, 6 out of 13. With the specific instruction *c,* all *O*s in both groups reported the presence of colors and described them.

TABLE I

NUMBER OF *O*s REPORTING COLOR UNDER DIFFERENT INSTRUCTIONS

Instruction	Knowledge	Colors reported	Colors not reported
a	with	7	2
	without	4	9
	total	11	11
b	with	7	2
	without	6	7
	total	13	9
c	with	9	0
	without	13	0
	total	22	0

Under all three of the instructions, and in particular under instruction *c,* all the primary and intermediate hues were reported by some of the *O*s, although generally not as appearing at one and the same time. The hues were usually of low saturation and light tint. Sometimes, however, saturated colors were reported and also spectral distributions. Descriptive terms for the latter were "small rainbows" and "the sort of thing you see when oil is on water." As a rule, however, only one or two colors were seen at a time or by a given *O*. Three *O*s reported blue alone and one reported yellow alone. The most frequent report was of yellow and blue. Indeed, in approximately 75% of the experiments, the first color reported was yellow or blue, or yellow and blue together.

In general the colors were observed in diamond-shaped patterns in the plane of the paper at right angles to the diagonals. Sometimes they appeared as nodules in the white spaces between the black lines, which then were wavy. A few instances were reported in which the colors appeared as dots over the pattern.

(2) Phenomenal movement. Phenomenal movement was reported under instruction *a* by 18 *O*s. In the main, two types of movement were described: (1) movement of particles over the figure as a whole and (2) movement of the lines in the pattern. Movement over the figure as a whole is characterized as follows: "rapid streaming of particles at right angles to the diagonal lines of the figure;" "shimmering or flowing;" "flickering like heat waves over the figure;" "specks as if it were raining;" "movement like the swirls on water when a pebble is dropped in a quiet pool." Movement of the lines in the pattern is characterized as "wavy movements of the black lines" or "the black lines seem to wiggle."

Under instructions *b* and *c*, these movements were reported less frequently. In a few instances, however, a third kind of movement was reported. The pastels themselves moved, *e.g.* "streaks of blue moving around" or "running or blowing of the colors."

The effect of instruction is again evident. Under *a*, a general instruction to inspect the figure, the *O*s reported everything that they perceived, and movement was one of the more striking phenomena. Under *b* they were primarily concerned with the task of keeping their fixation beyond the plane of the stimulus-figure and their reports were fragmentary, being chiefly comments upon the difficulties of far fixation. Under *c*, the instruction to observe the pastels, the *O*s concentrated upon the colors and were little concerned with details of the stimulus-figure or other attendant phenomena.

In view of the results obtained in Part I, we used in the succeeding parts of the study *O*s with knowledge and only the instruction to observe and to report specifically upon the colors.

II. Effect of the Color of Pattern and of Background

The second part of our study concerned the effect of the color of the stimulus-pattern and of the background. The pattern was printed in red, yellow, green, blue, black, and white ink upon red, yellow, green, blue, black, neutral gray, and white backgrounds.[39] It was also printed in black on various kinds of white paper, viz. tissue, bond, egg-shell, English finish, and coated. The figures were centered on 6 x 9 in. backgrounds.

Procedure. These various combinations were exposed as in the experiments of Part I, to 6 of our most highly trained and competent *O*s.[40]

Instructions. The following instructions were given to *O* at the beginning of every experimental period.

> "Look at the stimulus-pattern and describe in detail the colors observed and the appearance of movement of any kind."

Results. (1) Color. The 'subjective' colors were most pronounced with the following combinations of pattern and background: (1) black diagonal lines on white background; (2) blue on green or white; (3) black on green or yellow; and (4) green on yellow. In no case were pronounced color-effects produced with yellow or white diagonals. Many of the com-

[39] The colored backgrounds were the primary shades of the Milton Bradley papers.

[40] Miss L. Bestor, graduate student in psychology; Mr. J. D. Coakley and Mr. W. S. Neff, assistants in psychology; Dr. A. D. Glanville, research fellow in psychology; and the authors.

binations yielded no 'subjective' colors and some only different shades and chromas of the background color. The best color combinations of figure and background produced less pronounced effects than black on white.

The range or variety of colors reported was about the same as in Part I. Their shapes, however, were more varied. Four different patterns were reported: (1) a reticular pattern, *i.e.* the diamond-shaped pattern reported in Part I; (2) irregular areas or blotches of pastel colors ill-defined in outline or form; (3) bands, streaks, zig-zag and waving forms; and (4) figures in which the interstices between the diagonal lines are striated, nodulated, or beaded with color.

(2) Movement. Movement of some kind seemed to be inherent in the perception. No less than six types were reported by the *Os*.

(a) Streaming. The most frequent type, reported by all the *Os*, was the streaming movement, a "movement of small particles in direction downward to the left at right angles to the diagonals." The movement was like "the drift of a very fine snow," and its rate varied from "a slow drift" to "a rapid streaming." This type of movement did not involve the 'subjective' colors but was a movement of fine particles, white or gray in quality, through which the stimulus-pattern and the 'subjective' colors were seen.

(b) Random movement. A second class of movement was described by the *Os* as a "movement of a shadow or cloud," "fluctuation of lighter and darker areas," "moving blobs, indefinite both as to shape and direction," "clouds of color floating over the figure." This type involved colors as well as grays and was random over the figure without definite direction.

(c) Moving bands and zig-zag lines. In addition to these movements of irregular areas, movements of bands and zig-zag lines were described. For example: "jagged designs that fluctuated in position," "fluctuating zig-zag lines," "waving bands of color," "iridescent bands which moved over the stimulus-figure, never constant nor well-defined in direction or in form," "bright and dark bands, which oscillate and follow the direction of the 'streaming' movement"—described in (*a*) above.

(d) Movement of stimulus-lines. The lines of the stimulus-pattern often themselves exhibited movement. They were reported as "moving violently," "wriggling slightly," "approaching one another and then swinging apart again rapidly, giving the appearance of a body of water during a heavy storm," and as "breaking up into dashes and moving against the 'streaming' movement," as "waving," and as "curling."

(e) Circular movement. A fifth kind of movement was circular. "Dark gray and blue splotches move around the center of the pattern," "a circular movement in the field," "a rapid movement of large gray areas, approximately circular." This type of movement is like that described in (*c*) in that the moving pattern is ill defined, but it differs in the fact that the direction of movement is well defined.

(f) Shimmer, seething. In addition to these five types, still another is discernible. It is the most indefinite and vaguest of all. It is movement without something definitely moving and is described as a "wave-like shimmering of the whole pattern," a "violent seething in all directions," as "movement similar to that observed when a pattern is observed through heat waves."

The moving patches, zig-zag lines, and bands were frequently reported as colored. The color frequently, though not always, conformed to the hue of the background of the stimulus-pattern. Blue and gray were notable exceptions. Moreover, the zig-zag lines in particular failed to conform to the quality of the background, though they were perhaps to some extent at least influenced by it.

The stimulus-patterns most favorable to the perception of movement, as judged by the rapidity, amount, and number of kinds of movement reported, were (1) black figure on white background, (2) red on white, (3) blue on white, (4) black on yellow, and (5) white on blue. The combinations least productive of movement were, in general, those with white or yellow figures. As a rule, the paler and less pronounced the figure, the less likely is the pattern to produce phenomenal movement. With one exception (white figure on blue background), the best combinations were dark lines and light backgrounds; in other words, those most nearly approaching the optimal combination of black figure on white background.

The movement phenomena reported by our *O*s are similar to, but not identical with, Ferree's "streaming phenomenon."[41] The chief difference is that our movements never blotted out the stimulus-pattern, but were superimposed upon it.

(3) Color and movement. No direct relation obtains between the insistence and variety of the 'subjective' colors and the amount and diversity of the perceived movement. Some of the combinations, *e.g.* yellow figure on gray background, were equally poor for color and for movement; some, *e.g.* blue figure on green background, were equally good for both; and some *e.g.* white figure on blue background, were poor for the pastels and good for movement. No combination yielded good pastels and poor movement. On the whole, all the color combinations of figure and background were highly productive of phenomenal movement and poor in the production of the pastels. None gave pastels of the qualitative range or brilliancy given by the black figure on the white background.

(4) Paper texture. The texture of the paper in the black-white series (black figure on tissue, bond, egg-shell, English finish, and coated stock) had, under our conditions, no discernible effect upon the *O*s' reports. Experiments with these stimulus-combinations were scattered haphazardly among the others. They all appeared, in contrast to the color-combinations,

[41] C. E. Ferree, The intermittence of minimal visual sensations, this JOURNAL, 19, 1908, 114-129; The streaming phenomenon, *ibid.*, 484-503.

to be equally good in yielding the phenomenal effects, viz., colors and movements.[42]

III. Effect of Line-Width and of Spacing

Having found that the black-white combinations were the most favorable for the arousal of the 'subjective' colors, we next turned to discover the optimal width of the pattern-lines and of the intervening spaces.

Procedure. A series of stimulus-patterns, similar to the Luckiesh-Moss in size and form was prepared by varying width of lines and spaces systematically. The figures, drawn in India ink, were centered upon 6 x 9 in. white cards. Five line-widths (0.5, 1, 2, 4, and 6 mm.) and five space-widths (1, 2.5, 5, 7, and 9 mm.) were used. As every line-width was combined with every space-width, there were in all 25 stimulus-patterns in the series. With the exception of the stimulus-patterns, procedure, *Os*, and instructions were the same as in Part II.

Results. (1) Color. The results were unequivocal. Narrow lines in combination with narrow spaces yielded numerous and brilliant pastels; narrow lines and wide spaces, or broad lines and narrow spaces, yielded few and very unsaturated pastels; and broad lines with wide spaces yielded none. Under our conditions, the wider the lines and the larger the interstices, the fewer and the less saturated were the 'subjective' colors. For example, when the optimal line-widths (0.5 and 1 mm.) were combined with the optimal space-widths (1 and 2.5 mm.), the pastels covered a wide range of quality and brilliancy. When, however, the optimal line-widths were used with the larger space-widths or the optimal space-widths with the larger line-widths, either no colors were perceived or the colors were very desaturated and evanescent, fading quickly. When line-widths of 4 and 6 mm. were combined with space-widths of 7 and 9 mm. no colors were seen.

The color qualities reported in this part of the study were similar to those reported in Part I c. Of the stimulus-patterns yielding color, yellow and blue were reported together in 43%, yellow alone in 34%, and blue alone in 8%. In addition to yellow and blue, red and green were reported together in 7.5% of the patterns and red alone in 7.5%.

(2) Movement. Phenomenal movements were observed only with the narrow lines and spaces. The conditions effective in this series of experiments for the perception of pastels were also effective for the perception of movement. Some indication was obtained, however, from the non-optimal patterns that the phenomenal movements depended upon eye movement. One *O* (*C*) reported, when wide lines and narrow spaces were used, that "the entire card seemed to vibrate . . . and in

[42] It is quite possible, had the black-white combinations been presented in a series by themselves, that some textures would have yielded more pronounced effects than others, but that was a variation in procedure that we did not pause to consider.

unison with eye movements." Again, when wide lines and wide spaces were used, "movements occurred only when I moved my eyes."[45]

IV. Effect of Eye Movement

Having determined the optimal instructions and the optimal quality and form of the stimulus-pattern, we turned to discover the effect of eye movement.

Procedure. The Luckiesh-Moss pattern printed in dead black on egg-shell white paper—the pattern used in Part I—was presented to the *O*s in a modified Dodge tachistoscope.[44] The exposure-times, controlled by means of a condenser-charge apparatus,[46] were: 45, 115, 382, 455, 522, 752, and 873 ms. and 1, 2, 3, 4, and 5 sec. The fixation-point was placed, in all but a few experiments with one *O*, in the plane of the stimulus-figure. With the exceptions described, the procedure was the same as in Part II.

Results. Since eye movements may be made during exposure-times longer than 100 ms., the possibility of our *O*s moving their eyes was ruled out in only one of the intervals—45 ms. Not only were no 'subjective' colors reported by our *O*s in that interval, but they were not reported in any interval shorter than 3 sec. In intervals of 3 sec. and longer, they were reported by all our *O*s. That there is a dependence of the 'subjective' colors upon eye movement cannot, in face of these results, be questioned; but the fact that exposure-times capable of yielding eye movement did not yield color indicates, as we believe, that eye movement is not the only condition necessary for the production of color.[48]

The few experiments with near and far fixation yielded 'subjective' colors during shorter exposure-times than in the other experiments with fixation in the plane of the stimulus-figure. The *O*s and the experiments were too few in number, however, to warrant generalization. It is, never-

[45] Another interesting observation was made by *D*, at first with wide lines and spaces and then, after the phenomenon had caught his attention, with the narrower lines and spaces but never with the narrowest lines and spaces. The observation was that "the stimulus-patterns possess tridimensionality, the diagonal black lines look as ribbons projecting at right angles to the page."

[44] J. G. Jenkins, A simple tachistoscope of many uses, this JOURNAL, 45, 1933, 150. Cf. also Raymond Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10-13.

[46] W. D. Moeder and J. G. Jenkins, A condenser-charge timer, this JOURNAL, 45, 1933, 742-744.

[48] We do not know whether eye movements were actually made during the briefer exposures; all we know is that they might have been made, and that it is highly improbable that they were not made at least during the intervals as long as 1-2 sec. This part of our study should be repeated with a recording of eye movement during the exposures. The recently devised method of recording eye movement by means of the electrical potentials of the muscles of the eyes offers an excellent means of accomplishing this end.

theless, worthy of note that this result corroborates those obtained in Part I b, in which fixation was beyond the plane of the stimulus-pattern. The phenomenon seems to be enhanced by fixation outside of the plane of the stimulus-figure. This may be due to movements—of convergence, accommodation, or both—that are involuntarily made to bring fixation into the plane of the figure. The relative disadvantage of fixation in the plane of the figure may after all, then, be sheerly a matter of eye movement.

SUMMARY

(1) A pattern of narrow black lines close together on a white background gives rise, when regarded steadily for short periods of time, to the perception of pastel colors—with or without prior knowledge and specific instructions to look for them.

(2) The colors are usually of low saturation and of light tint. The hues, while predominantly yellow and blue, cover the entire spectral range.

(3) The colors are enhanced when fixation is not in the plane of the figure.

(4) The colors appear in four positions upon the stimulus-card: (a) perpendicular to the diagonal lines of the figure in diamond-shaped patterns; (b) in bands or zig-zag lines, also perpendicular to the lines of the pattern; (c) in ill defined masses or clouds; and (d) in the spaces between the black lines.

(5) While some stimulus-patterns of colored figure and background give rise to the perception of the 'subjective' colors, no combination was found which approximates the black on white combination in effectiveness.

(6) Widening the diagonal black lines and increasing the intervening white spaces decreases the effect.

(7) Six kinds of phenomenal movement are reported: (a) streaming, (b) random, (c) fluctuating zig-zag lines and bands, (d) movement of the stimulus-lines, (e) centrifugal or circular, and (f) shimmering or seething.

(8) The 'subjective' colors are to some extent dependent upon eye movement, but eye movement, though necessary, does not appear to be a sufficient or compulsory condition.

THE SPECTRAL LOCATION OF PSYCHOLOGICALLY UNIQUE YELLOW, GREEN, AND BLUE

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Color vision has had as much attention, both experimental and theoretical, in scientific literature as any other single subject. One might expect to find an adequate answer to almost every question in the field he might raise; but the assumptions upon which the various experiments have been based are widely varied, and the theorization that has been offered has been conceived with specialized aims. As a consequence important problems have been obscured or have been passed by on either side because they happened not to lie within the theoretical scope of investigations which might be expected to include them. The particular problem with which we are concerned is such a one; namely, the specification, in experimental terms, of the primary or unique colors.

Seven points of reference combined in an equation have been proposed as necessary and sufficient for the psychological description of all possible colors.[1] Red, Green, Yellow, Blue, and White have been generally accepted as necessary to the qualitative description of colors, but questions have been raised about black and gray. Black, however, is the *psychological* as well as the logical complement to white, and experimentation has shown it to be a positive color quality, necessary as a point of qualitative reference.[2] Helmholtz's comment is interesting in this connection: "Black is a real sensation, even if it is produced by entire absence of light. The sensation of black is distinctly different from the lack of all sensation."[3] Southall reaffirmed this position: "The physicist is right in thinking of a black object as sending no stimulus to the retina, but he is wrong when he assumes that there is no black sensation correlated with this absence of stimulation. It is absurd to say that black is the absence of visual stimulation. Black is a positive sensation just like any other visual sensation, and just as real and distinct as the sensation of white or of yellow and blue or of red and green."[4]

* Accepted for publication November 15, 1938.

[1] F. L. Dimmick, A reinterpretation of the color pyramid, *Psychol. Rev.*, 36, 1929, 83-90.

[2] E. B. Titchener, A note on the sensory character of black, *J. Philos., Psychol., & Sci. Methods*, 13, 1916, 113-121; A further word on black, *ibid.*, 649-655; F. L. Dimmick and C. H. Holt, Gray and the color pyramid, this JOURNAL, 41, 1929, 284-290.

[3] H. Helmholtz, *Physiological Optics*, 2, 1924, 131.

[4] *Ibid.*, 60 (footnote 1).

The question of gray, raised more recently,[5] points to a similar treatment of that color, although it has certain aspects that set it off as distinct from the other six colors: to wit, it has no complement, it combines with any color, and as Titchener[6] and Stumpf[7] have said, it may be taken as the zero-point of the "pro-intensive" series.

The standards for black and white have been fairly well established: for white a magnesium oxide surface, viewed at an angle of 90°, with heterogeneous light striking it at an angle of 45°, and observed through a reduction screen of very low reflectance;[8] for black the color of lightless space, obtained by looking through an aperture into a box, the interior of which is non-reflecting.

The standard middle gray has been specified experimentally in terms of the proportion of two reflecting surfaces.[9] The equivalent luminance of this standard is $5 \pm .5$ foot lamberts or $0.011 \pm .001$ candles per sq. in. measured by the Luckiesh-Taylor Brightness meter.[10]

When we seek comparable standards for the remaining four primary colors, however, we do not meet with similar success.[11] We find a large amount of data, little of it substantiating any part of the remainder—data obtained by different methods, on different assumptions, and for different purposes. There is relatively little agreement even as to a procedure which is adequate to establish an experimental standard for primary colors.

Hering was well aware of the necessity for distinguishing the psychological primaries from the physiological processes of the retina and from the Maxwellian mixture primaries and has characterized the three principal concepts pertaining to primary colors, concepts which too often we find confused and interchanged.[12] While few authors fall neatly into any one of his categories, it is well to recognize the separate concepts which are involved in the various discussions of primary colors. Hering himself does not maintain the consistent separation of his concepts that

[5] F. L. Dimmick, A note on the series of blacks, grays, and whites, this JOURNAL, 31, 1920, 301-302; The series of blacks, grays, and whites, *Psychol. Rev.*, 32, 1925, 334-336.

[6] Titchener, Visual intensity, this JOURNAL, 34, 1923, 310.

[7] C. Stumpf, *Die Attribute der Gesichtsempfindungen*, 1917, 20, 27, 32, 72.

[8] D. B. Judd, Preparation and colorimetric properties of a magnesium-oxide reflectance standard, *Nat. Bur. Stand.*, LC-395, 1933, 1-4. The reference here is not to white light but to a "seen" white light, or to white as a color quality.

[9] F. L. Dimmick and George McMichael, The psychological determination of the limits of pure gray, this JOURNAL, 45, 1933, 313-314.

[10] M. Luckiesh and F. K. Moss, *The Science of Seeing*, 1937, 323; M. Luckiesh and A. H. Taylor, A brightness meter, *J. Opt. Soc. Amer.*, 27, 1937, 132.

[11] Judd, Hues of the spectrum colors, *Nat. Bur. Stand.*, LC-454, 1935, 1-6.

[12] E. Hering, *Grundzüge der Lehre vom Lichtsinn*, 1, 1907, 2-3; Prüfung der sogenannten Farbendreiecke, *Arch. f. d. ges. Physiol.*, 47, 1890, 425.

many citations of his work would lead one to expect. He had a definite aim for his theorizations and the original distinction was made for the purpose of leading up to his systematic conclusions. That accomplished, there was no value, for him, in insisting upon the cleavage between the concepts. Consequently we find him defining *Urfarben* as an obviously psychological term,[13] but later using it in a physiological sense, for those colors which do not shift in hue with a change from foveal to peripheral vision.[14] The two are descriptive of different phenomena, the one of a psychological judgment, the other of a retinal condition; but agreement between the two was valuable for Hering's theory. He did not, however, hesitate to point out the disagreement between similar facts where they did not support his theory; for example, the disparity between the mixture complementaries and the psychological primaries. The same lack of a consistent separation of concepts is found throughout the literature and seriously impairs the reliability of various standards given for the primary colors.

In Table I,[15] we have summarized the values given in the literature, but direct comparison of those of one author with those of another is not valid, as will be obvious from the following discussion.

(1) Helmholtz gives the spectral location of the primary colors and some of the non-primaries, but this specification is not exact and there is no description of the experimental procedure involved. For example, he says: "Red is the color at the less refrangible end of the spectrum, which shows no marked difference from the extreme end of the spectrum to about line E. . . . From the line C to the line D the red passes through orange . . . into golden yellow. . . . There is a rapid transition of color from the line D to the line b. First there is a narrow strip of pure yellow which lies about three times as far from E as from D. Then comes green-yellow, and finally, pure green between E and b. Between the lines E and F the green becomes blue-green and blue, and from F to G there are different shades of blue. . . . Violet is the region in the spectrum from the line G to the line H or L."[16] These descriptions can, of course, be translated into mμ, but it is evident from his treatment of them that Helmholtz does not intend them to be more than a matter of obvious, careful inspection without experimental controls. Later writers have often referred to Helmholtz as if he gave exact specifications of the principal colors, but they cannot have correctly interpreted Helmholtz's intention.

(2) Von Bezold,[17] for example, follows Helmholtz's specifications exactly, and offers no further substantiation of those values.

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[13] Hering, *Grundzüge* (*op. cit.*), 40-43; Zur Erklärung der Farbenblindheit, *Lotos*, 1, 1880, 81 ff.

[14] Hering, *op. cit.*, *Lotos*, 82; Prüfung der sogenannten Farbendreiecke, *op. cit.*, 423.

[15] Judd, Hues of the spectrum colors, *op. cit.*

[16] Helmholtz, *op. cit.*, 64 f.

[17] Wilhelm von Bezold, *Theory of Color* (Trans.), 1876, 20-22.

(3) Donders,[18] in 1884, made a careful determination of unique yellow. He required of his *Os* a short period of so-called "neutral" adaptation in a room illuminated only by light from a cloudy sky falling through a 'white' ground glass window. The experiment was performed on 111 eyes, but Donders does not state how many reports were obtained from every *O*, nor does he follow a precise method in obtaining the judgments, but, instead, what he calls a "slow backward and forward motion," decreasing in range, in the region of the 'pure yellow.' With this

TABLE I

SUMMARY OF THE VALUES (IN mμ) GIVEN BY THE VARIOUS AUTHORS FOR RED, YELLOW, GREEN, AND BLUE

Author	Date	Red	Yellow	Green	Blue
(1) Helmholtz	1866	686.9	589	526–516	486
(2) Bezold	1874	686.9	589	526–516	486
(3) Donders	1884		582		
(4) Hess	1888	PR*	574.5	495	471
(5) Hering	1890	PR*	577	505	470
(6) König and Dieterici	1893	PR*		505	470
(7) Voeste	1898		560	490–500	460
(8) Ladd-Franklin	1902	PR*	576	505	470
(9) Exner	1902	494(C)		508	475
(10) Von Kries and Schottelius	1908	606.6	574.4	503	
(11) Westphal	1910	445+655†	575.5	516	478.8
(12) Dreher	1911		575	509	477
(13) Ridgway	1912	644	577	520	473
(14) Goldytsch	1917	692‡			467
(15) Allen	1919	680		570–470	420–400
(16) Goldmann	1925		568	505	468
(17) Priest	1926	680	583	515	475
(18) Brückner	1927		578	498	470
(19) Schubert	1928		574	500	467
(20) Maerz and Paul	1930	644	585	521	452
(21) Purdy	1931		576	504	476
(22) Hecht	1931	650–700		505–500	470
(23) Weld	1932	624 599	571 496	454	
(24) Ornstein, Eymers, and Vermeulen	1934	630	581–576	555–502	487
(25) Verbeek and Bazen	1935	667–646	580	536	480–477.5

* PR indicates that the author states that all spectral reds are yellowish or that red lies toward purple from spectral red. (C) gives the complementary wave length to non-spectral "pure" red.

‡ A mixture of these two wave lengths give a true red.

† 692 has 11.5% yellow.

procedure there is a greater opportunity for successive contrast to be effective, as he notes. Donders had a definite idea of the difficulty of a color judgment of this type, for he does not require his observers to indicate a 'pure' yellow, but to judge the limits of yellow, *i.e.* that point where it becomes noticeably reddish or greenish, and he calculates the unique yellow as the mean between the two. Donders' experiment seems more adequate than most, in spite of certain uncontrolled

[18] F. C. Donders, Farbengleichungen, *Arch. f. Anat. u. Physiol.*, Physiol. Abt., 1884, 518 ff.

factors, because it is dependent upon a single experimental concept, that of a yellow that is neither reddish nor greenish. Primary yellow is thus defined as that yellow which, under the conditions outlined above, corresponds to 582 mμ on the average with extremes of the range at 472 mμ and 594 mμ.

(4) Hess,[19] worked with Hering and, following Hering's theory of color vision, produced results in substantiation of it. The primary colors which he sought to establish through experiment are the so-called 'unveränderlich Farbentöne,' that is, those colors which change only in saturation and not in hue toward the periphery of the visual field. These primaries, which are physiological, were found by Hess to correspond to a red lying farther toward the purple than that of the extreme red end of the spectrum, a yellow of 574.5 mμ, a green of 495 mμ, and blue of 471 mμ. The experimental conditions under which they were obtained are not made explicit. The four primaries form two pairs of complementary hues, which agrees with Hering's contention. Hess implies that the same hues are also the psychological primaries, but there is no attempt to verify the statement.

(5) Hering fails to describe any experiments in which he systematically determined the spectral location of the *Urfarben*,[20] but in a footnote he cites 470 mμ for blue, 577 mμ for yellow, and a color slightly less yellow than 505 mμ for green, the red lying, as with Hess, farther toward the purple than the red end of the spectrum.

(6) König and Dieterici use the data of wave-length mixture to substantiate hypotheses regarding the physiology of the retina and the psychology of color quality.[21] They determined the mixture curves for their spectral primaries with 4 *Os*, 2 normal and 2 anomalous trichromats. From the points of intersection of these curves they deduced the Grundempfindungen. "Wir wollen nunmehr unter 'Grundempfindung' eine solche Empfindung verstehen, der ein einfacher (d.h. durch keine Art des Reizes weiter zerlegbarer) Prozess in der peripherie des Nervus opticus entspricht."[22] It is difficult to state whether the Grundempfindungen were also intended to be "simple sensations" *i.e.* psychologically unique colors. Something of the sort seems to be implied, but the relation of these concepts to one another and to the concept of *Urfarben* is not clear. The conditions of observation are not described but it is stated that each comparison was repeated at least ten times in the bright region of the spectrum, and at least twenty times in the dark or blue region.[23]

(7) Voeste,[24] in 1898, introduced a still different concept of invariable hues. He reported on the qualitative changes of spectral colors, between 660 mμ and 430 mμ attendant upon retinal fatigue. By matching light of a known wave-length projected upon a fatigued portion of the retina with light of the same wave-length projected upon an unfatigued area, he found three invariable points which showed

[19] C. Hess, Über den Farbensinn bei indirektem Sehen, *Graefe's Arch. f. Ophth.*, 35, 1889, 1-62.

[20] Hering, Prüfung der sogenannten Farbendreiecke, *Arch. f. d. ges. Physiol.*, 47, 1890, 425 ff.; Zur Erklärung der Farbenblindheit, *Lotos*, 1, 1880, 81 ff.

[21] Arthur König, and Conrad Dieterici, Die Grundempfindungen in normalen und anomalen Farbensystemen und ihre Intensitätsverteilung im Spektrum, *Zsch. f. Psychol.*, 4, 1893, 241-347.

[22] *Ibid.*, 324. [23] *Ibid.*, 296.

[24] H. Voeste, Messende Versuche über die Qualitätsänderungen der Spectralfarben in Folge von Ermüdung der Netzhaut, *Zsch. f. Psychol.*, 18, 1898, 257-267.

no shift in hue as a consequence of fatigue. These points correspond to 560 mμ, from 490 mμ to 500 mμ, and 460 mμ. Voeste used five intensities, and the time of observation of the fatiguing light was 10 sec. in all cases except with the lowest intensity where the time of fixation was 15 sec. Here again there is not sufficient information regarding the conditions of observation to permit the comparison of Voeste's data with those derived from other procedures.

(8) Ladd-Franklin has cited wave-length specifications for four primary colors,[25] but there is no indication that these values were obtained experimentally.

(9) Exner[26] published work supplementing that of König and Dieterici. His procedure is evidently identical with theirs, and his results deviate little from theirs. The experiment was carried out on 4 *O*s with normal color vision, and each measurement was repeated 10 times during a period of several days. No more is said of the actual conditions of the experiment, but Exner's work is considered an extension of that of his predecessors.

(10) Von Kries and Schottelius published some experiments in 1908 which, in part, bear upon this subject.[27] In the section of interest to us, the experimenters sought to establish values for what they call 'reine Prinzipalfarbe.' *O* was required to make judgments on the basis of the 'immediate sensation-impression' compared to a "pure color" standard previously shown. There seems to have been too much knowledge on the part of *O* of the nature of the experiment, too little control of the conditions of observation, and too few cases.

(11) Westphal says, in stating his problem:[28] "In dieser Untersuchung über die Urfarben sollen die Wellenlängen, die verschiedenen Personen zur Erzeugung von reinem Rot, Gelb, Grün, und Blau dienen, bestimmt werden. Es wurde davon ausgegangen, als reines Rot sei solches zu bezeichnen, das weder nach Gelb noch nach Blau neige, als reines Gelb ein solches, das weder nach Rot noch nach Grün neige, usw."[29] He is, then, setting his problem as psychological, and he follows this up with the instructions to *O* to give the judgment 'Urfarbe' at the point where the first trace of the new color component becomes visible. His account of the conditions of the experiment is more adequate than most. All observations were obtained between 9 A.M. and 1 P.M.; there were 10 *O*s, 3 of whom were color-blind; each value given seems to be the average of 4 'pairs' of presentations, one ascending and one descending series constituting a pair; the method used throughout was the method of limits, and there was no knowledge on the part of *O* except that essential to the method.

(12) Dreher investigated the invariable hues,[30] both with a change of intensity and with a shift from foveal to peripheral vision, but the two concepts were kept

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[25] Christine Ladd-Franklin, Vision, *Baldwin's Dict. Philos. & Psychol.*, 1, 1902, 767 f.

[26] Franz Exner, Über die Grundempfindungen im Young-Helmholtz'schen Farbensystem, *Sitz. d. Wein. Acad., math.-nat. Kl. 3*, 2a, 1902, 857-877.

[27] Lotte von Kries and Elisabeth Schottelius, Beitrag zur Lehre vom Farbengedächtnis, *Zsch. f. Sinnesphysiol.*, 42, 1908, 192-209.

[28] Hans Westphal, Unmittelbare Bestimmung der Urfarben, *ibid.*, 44, 1910, 182-230.

[29] *Op. cit.*, 182.

[30] Edgard Dreher, Methodische Untersuchung der Farbentonänderungen homogener Lichter bei zunehmend indirektem Sehen und veränderter Intensität, *Zsch. f. Sinnesphysiol.*, 46, 1912, 1-82.

distinct and investigated separately. The *Os*, 4 in number, served in from 4 to 20 trials on a single match. Light adaptation was maintained at all times and the time of observation was 1 sec. Dreher was careful to keep the exit slit of his apparatus small enough to insure a nearly monochromatic stimulus, and to obviate the influence of after-images and fatigue of the observer. He states that he followed the method of limits, but it appears that he used unequal steps, and that he often acted as both *E* and *O* at the same time. In this experiment he located the invariable hues. The values given in Table I are for the "subjektiv reine Farben," for which the procedure is not given. Dreher does not, however, equate the different types of primaries, as some others have done.

(13) Ridgway cites wave-length values for what he calls "the six psychologically distinct colors of the solar spectrum," and his statement has at times been referred to as defining primaries.[21] But there is no indication of how these values were obtained. Evidently it is a casual statement without experimental evidence.

(14) Goldytsch published an article in 1917 which is interesting from the point of view of method.[22] Using a Hering window he attempted to establish the yellow valence of spectral red in terms of the percent of 'pure' blue which must be added to the red in order to produce a contrast effect of 'pure' green. This procedure involves several assumptions which are of doubtful validity. The first is that the presence of yellow is more easily and accurately detected in green than in red. Our results, as well as those of most investigators, indicate that judgments of color are more difficult in the region of the green. The second assumption is that a contrast effect is more accurately judged than the original color. The last is that a unique color will necessarily produce a unique contrast effect, which is, in fact, assuming a one to one correspondence between the physiological and the psychological processes involved. He used 3 *Os*, and he fails to state how many trials were required for each value, or what psychophysical procedure was followed. The 'pure' blue which he added to the red he chose by a casual inspection of the spectrum. The judgment required of *O*, that of a 'pure' green, is too complex and difficult a judgment to require of an *O*.

(15) Allen compared curves of the persistency of vision throughout the spectrum, of an eye adapted to diffused daylight, with similar curves of an eye fatigued by light of specified wave-lengths.[23] The curves of the fatigued eye showed 5 types of elevations or deviations from the normal curve, depending upon the wave length of the fatiguing stimulus, and four points of transition from one type of elevation to another where the normal and fatigue curves coincided. Three of the elevations he takes to be indicative of three fundamental color sensations because they involved in each case only a single region of the spectrum corresponding to red, green, and violet respectively. Allen's method is in some respects analogous to that of König and Dieterici, and of Exner, but he neglects to state the number of *Os* or the number of trials.

(16) Goldmann sought to establish the 3 Urfarben according to the following

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[21] Robert Ridgway, *Color Standards and Color Nomenclature*, 1912, 7.

[22] L. Goldytsch, Messende Untersuchungen über die Gelbvalenzen spektraler roter Lichter mit Hilfe einer neuen Methode, *Zsch. f. Biol.*, 67, 1917, 35-56.

[23] Frank Allen, On the discovery of four transition points in the spectrum and the primary color sensations, *Philos. Mag.*, 38, 1919, 55-81.

criteria:[84] (1) uniqueness of color for sensation; (2) no change of hue with a change of intensity; (3) no change of hue with a change to peripheral vision; (4) no change of hue with a change of saturation; and (5) the complementarism of hues in compensation and in successive and simultaneous contrast. Before the experiment there was a 10-min. period in the dark, and the duration of observation was 2 sec. There is no indication of the number of *Os*, but there seem to have been 'at least three' observations for each specification.

(17) The data of Priest are unpublished,[85] and were supplied by D. B. Judd of the Bureau of Standards.

(18) Brückner made determinations of the Urfarben as a preliminary to another experiment.[86] The criterion was that the color should appear 'neutral,' *i.e.* unique, for both direct and indirect vision. From 8 to 13 determinations were made for each of the 4 primaries, but no further data are given.

(19) Schubert investigated the Urfarben under various conditions of illumination.[87] His definition of Urfarben is much the same as that of Goldmann. He gives no data concerning the procedure in the experiment, but he found that the Urfarben, determined under specific conditions of illumination, were complementary under those conditions, and were not complementary under somewhat different conditions.

(20) Maerz and Paul cite wave-length specifications for several colors,[88] none of them necessarily unique colors.

(21) Purdy made an investigation both of the invariable hues and of what he calls 'critical' or unique colors.[89] Using various intensities measured in photon units, he found 4 invariable points, one of them extra-spectral, in the purple-red. The spectral invariables are: 571 mμ, 506 mμ, and 474 mμ. He then made a determination of the Urfarben as defined by Westphal. These values have been given in Table I. We have here the advantage of having both types of data determined under the same conditions and by the same observer. Purdy finds a close measure of agreement between the two, the discrepancy being largest in the case of yellow. The physical conditions under which the investigation was carried out were very carefully controlled, but Purdy fails to describe his psychological procedure.

(22) Hecht follows the König-Dieterici assumption in regard to color specification, with no more specific information concerning procedure or conditions.[90] After a very careful consideration of a great variety of experimental data, he states that it is virtually impossible to utilize the curves of "color" mixture to represent anything other than "color" (stimulus) mixture.[91] He admits, however, that König

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[84] H. Goldmann, Über Dauerwirkung farbiger Lichter auf das Auge, *Arch. f. d. ges. Physiol.*, 210, 1925, 70-115.

[85] Judd, Hues of spectrum colors, *op. cit.*

[86] A. Brückner, Zur Frage der Eichung von Farbensystemen, *Zsch. f. Sinnesphysiol.*, 58, 1927, 322-361.

[87] G. Schubert, Sind urfarbige Lichterpaare komplementär? *Arch. f. d. ges. Physiol*, 220, 1928, 82-100.

[88] A. Maerz and M. R. Paul, *A Dictionary of Color*, 1930, 5.

[89] D. M. Purdy, Spectral hue as a function of intensity, this JOURNAL, 43, 1931, 541-559.

[90] S. Hecht, The interrelation of the various aspects of color vision, *J. Opt. Soc. Amer.*, 21, 1931, 615-639.

[91] Hecht, The development of Thomas Young's theory of color vision, *ibid.*, 20, 1930, 237 ff.

thought of the mixture primaries as corresponding both to the fundamental color sensations and to the physiological primaries, and states that there is a tacit agreement within the field that this corresponds to the actual facts. Such a statement brings one of the most pressing of our problems into the open at last and indicates the need for experimental data to exchange for present assumptions.

(23) Weld attempted to determine statistically the wave-length boundaries between the "six spectral colors."[42] His 38 *Os*, most of them casual visitors to the laboratory, were asked to set the cross-hairs of a spectroscope ocular on what they considered the boundaries between red and orange, orange and yellow, etc. In regard to his general methodology Weld says: "The writer can think of no better basis of doing this [*i.e.* of standardizing the wave-length boundaries between the six spectral colors] than to take the average judgment of a sufficient number of people." Lewin has made the point that a statistical treatment of data can add nothing to the validity of the data themselves,[43] and Weld's data do not appear to be highly valid. As Luckiesh and Moeller pointed out in an investigation of the naming of common signal colors, "color names are by no means standard for a group of persons, even though the colors are common ones of fair saturation, and the names assigned are often different when the color is viewed singly than when it is viewed among a group of other colors."[44] In addition several of Weld's *Os* were color-blind.

(24) Ornstein, Eymers, and Vermeulen obtained results which, while they do no pretend to be exact, are interesting.[45] They simulated rail-road conditions, using a flash of color to the dark-adapted eye, and found very little agreement in the naming of colors, but this is of course due in part to the fact that, their problem being a practical one, they used relatively uncontrolled conditions.

(25) Verbeek and Bazen investigated the distribution of the 'chief' colors of the spectrum.[46] One hundred *Os* were shown a series of lights of 21 wave-lengths twice, *i.e.* in both spectral orders. They were asked to look at a stimulus, after dark adaptation, and to tell its color, in the following terms: "Red, orange, yellow, green, blue, white, gray and doubt." While they found certain points of agreement, the results were in general exceedingly scattered.

Thus we find in the literature that there is relatively little agreement upon the procedure appropriate to such an investigation. Several distinct concepts are employed without due cognizance of the fact that they are divergent. The question of the principal or unique colors has not been attacked as a single experimental problem and with a controlled psychological methodology. In view of the incompleteness of these earlier attempts,

[42] L. D. Weld, Some observations on spectral color discrimination, *Proc. Iowa Acad. Sci.*, 39, 1932, 209 f.

[43] Kurt Lewin, The conflict between Aristotelian and Galileian modes of thought in contemporary psychology, *J. Gen. Psychol.*, 5, 1931, 141-175.

[44] M. Luckiesh and J. W. Moeller, Naming the common signal colors, *J. Opt. Soc. Amer.*, 13, 1926, 465 ff.

[45] L. S. Ornstein, M. J. G. Eymers, and D. Vermeulen, Farbenerkennungsprüfungen mit Rücksicht auf die Brauchbarkeit von Signalgläsern, *Proc. kon. Akad. Amst.*, 37, 1934, 385 ff.

[46] H. P. J. Verbeek and M. L. Bazen, Distribution of the chief colors of the spectrum, *Physica*, 2, 1935, 380 ff.

we undertook to make an experimental determination of the spectral location of the psychologically unique colors.

EXPERIMENTS

Apparatus. The instrument used was a double-prism spectroscope fitted with an auxiliary eye-piece to afford *O* a Maxwellian view—the exit slit necessary to insure an apparently monochromatic stimulus was too narrow for a proper judgment of hue. This slit (0.13 mm. wide) passed a band 6 Å wide at the red end of the spectrum, whereas, according to Jones,[11] nowhere in the spectrum are differences of less than 10 Å distinguishable. The drum of the spectroscope was calibrated in millimicrons and accurate settings of 0.5 mμ could be made. The source of light was a 300-w., 115-v. Mazda projection bulb, rated at 24 lumens per watt and a color temperature near 3000°K. The entrance slit was set at 0.01 mm. in width.

Table II shows the intensity-values of the stimuli in the regions of the spectrum under examination.

TABLE II

INTENSITY-VALUE OF THE STIMULI

Wave-length	Band width (mμ)	Intensity (Ft. lam.)	Pupil diam.* (mm.)	Photons
582	.6	9	3	872
515	.45	5	4	798
475	.25	1.6	5	432

* The diameter of the pupil was estimated by inspection but our values approximate those obtained by Reeves' photographic method. (Cf. P. Reeves, Rate of pupillary dilation and contraction, *Psychol. Rev.*, 25, 1918, 330–340.)

A head rest kept *O*'s head in a constant position relative to the ocular. *O* was screened from direct light, as well as from a view of any of the apparatus except the ocular. The experiment was carried out in a dark room, illuminated by an Ivanhoe North-Light Lamp of 500 w., so that the general illumination was constant and dark-adaptation was avoided. Results were obtained from ten observers, three men and seven women, using foveal vision of the right eye. All the observers had normal color vision. The *O*s were: D. D. Scovil, P. Wolz, elementary students in psychology; E. F. Faust, K. Fitch, F. Brent, E. Brown, N. H. Ludlow, W. McClaren, advanced students in psychology; Dr. C. C. Dimmick and Dr. F. L. Dimmick.

Procedure. The procedure we followed answered the two requirements: (1) that *O*'s judgment should be simple and unequivocal, and (2) that it should not be affected by any color standard outside this experiment, such as a "good green," etc. Therefore, *O* was never required to judge the presence of the color under investigation, but was asked to report the point of first appearance of the bordering color, separate determinations being made for each border of a single color. For example, in determining the location of unique yellow, the point of just noticeable redness in yellow was first established, and later the point of just noticeable greenness. In a like manner both borders of green, and of blue were investigated. The actual

[11] L. A. Jones, The fundamental scale of pure hue and the retinal sensibility to hue differences, *J. Opt. Soc. Amer.*, 1, 1917, 63-77, esp. 74.

location of the unique color could thus be assumed to lie between the two experimental values. The location of red could not be determined with this apparatus because, as is generally recognized, all spectral reds are yellowish.

TABLE III

BOUNDARIES OF THE YELLOW, GREEN, AND BLUE REGIONS FOR ALL *O*s

O		Red in yellow	Green in yellow	Yellow in green	Blue in green	Green in blue	Red in blue
F	av.	584.92	580.87	542.71	511.04	485.00	467.72
	h	.94	1.02	.58	.84	1.10	.81
	σ	.75	.69	1.22	.84	.64	.87
CD	av.	584.40	577.48	500.65	492.50	481.23	471.63
	h	.74	.99	.71	.71	.64	.90
	σ	.95	.71	.99	.99	1.10	.78
S	av.	582.78	577.30	496.07	491.26	481.58	459.92
	h	.96	1.25	1.05	.91	.77	.84
	σ	.74	.56	.67	.78	.92	.84
W	av.	584.02	579.33	519.06	490.10	483.42	469.23
	h	.78	.89	.27	.88	.76	.63
	σ	.91	.79	2.62	.80	.93	1.12
L	av.	576.33	574.15	505.84	503.25	475.10	459.23
	h	.80	1.002	.68	.34	.64	.47
	σ	.88	.70	1.04	2.08	1.10	1.50
M	av.	582.25	579.63	539.11	506.10	486.88	464.61
	h	1.16	1.49	.46	.75	.91	.84
	σ	.61	.47	1.53	.94	.78	.84
FD	av.	593.01	584.32	536.01	513.27	477.93	466.95
	h	.72	.89	.29	.68	.71	.92
	σ	.98	.79	2.44	1.04	.99	.77
KF	av.	586.50	583.71	530.32	511.73	487.70	469.99
	h	1.16	1.09	.36	.64	.61	1.11
	σ	.61	.65	1.96	1.10	1.16	.64
FB	av.	593.62	579.99	557.72	510.75	489.37	474.82
	h	.83	1.07	.38	.38	1.22	.54
	σ	.85	.65	1.86	1.86	.58	1.31
BB	av.	584.79	576.97	538.70	513.01	480.28	469.18
	h	1.12	1.01	.39	.42	.69	.78
	σ	.63	.70	1.81	1.68	1.02	.91
Av.		585.26	579.36	526.62	504.31	482.25	467.32
m.v.		3.58	2.32	16.69	5.06	3.73	3.72
Location of unique color		Yellow		Green		Blue	
by average		582.35		515.45		475.1	
by median		582.7		514.6		476.5	

The final values were obtained by the Method of Constant Stimuli, preceded by a considerable amount of practice with the Method of Limits in which a high degree of stability of judgment was set up in the *O*s and the regions of change were roughly determined.[46] At the beginning of every observation period *O* was instructed

[46] E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280-293.

as follows: "After a ready signal, you will be shown a color. You are to judge whether that color is reddish or not-reddish." Several practice judgments which were not included in the calculations were taken at the start of every observation hour. Every determination is based upon 100 judgments on each one of seven equally spaced stimuli which had been found by preliminary work to lie in the region of doubt. The size of the stimulus-steps was 5 Å. Rest periods were given every 10 min., and observation periods were limited to 1 hr.

Results. The results are shown in Table III. The limits of each one of the three regions are given for every *O* with the two measures of reliability, h and σ. The average limits for all the *O*s are calculated, together with their mean variations. Finally, the mean of each region is taken as the most representative or 'best' yellow, green, and blue. These last values we obtained both by averaging the calculated limits of the regions and by taking the median J.N.D. step obtained from Jones's tables.[49] The two determinations do not differ significantly due to the fact that the stimulus-values of the J.N.D. remain fairly constant. Since the stimulus-difference for a J.N.D. by juxtaposed comparison is never less than 1 mμ we may state the three standards in the nearest mμ for convenience as yellow, 582; green, 515; blue, 476.

Although the limits of the regions show variations from *O* to *O*, the general conformity is striking. It bears out our contention that the human *O* may be used as an instrument, and will give reliable and accurate results. Care must be taken, however, in the standardization of the *O*s and the control of the conditions of observation. The greatest individual variations are due to conditional factors that could be eliminated on the basis of information gained in the course of the experiment. In every case but one, *O*'s average deviation about the limit of a region was no more than 2 J.N.D. of the juxtaposed comparison type, which have smaller values than judgments indicating the direction of difference. The greater variability in the yellow-in-green region involves at least two factors: (1) this region lies near the peak of the sensitivity curve, and the *O*s reported difficulty in distinguishing between the greater brilliance and the appearance of yellow; (2) since the J.N.D. in this region are 2 or 3 times as large as those in the other regions, the steps of 0.5 mμ were too small, decreasing the relative stability of our *O*s. The magnitude of the errors did not, however, appear to us sufficient to warrant the repetition of the determinations at this time.

When we compare our results with those of earlier investigators (Table I) it is evident that we agree most closely with those who, as nearly as

[49] Jones, *op. cit.*, 74.

we can judge, were attacking the problem from the same angle. This is notably true with respect to Donders, Westphal, and Priest.

The averages which Judd obtained from the values cited by various authors,[50] lie close to the points found in our experimental determination. While this correspondence seems to indicate the presence of common factors, the validity of those averages depends upon the assumption that such common factors existed, and until we had experimental confirmation of that fact, the assumption was only a lucky guess. One would scarcely have been justified in basing further work on so uncertain a basis. Such, indeed, was the situation that gave rise to the present investigation.

Likewise, it cannot be assumed that the spectral locations of the several unique colors will coincide either with those of the mixture complementaries or of the invariable hues. It is well known that the complements of red and green are both perceptibly bluish.[51] Helmholtz's value for the complement of spectral red, which is decidedly yellowish, lies between our unique green and unique blue. Until we have determined the location of unique red in terms of spectral red plus unique blue we cannot carry the comparison further. Our blue and yellow do not coincide exactly with Helmholtz's complementaries. Both lie to the red of their complements so that their mixture should leave a residual red comparable to the residual yellow from the mixture of unique red and green. This fact may have importance for the physiological hypothesis of color vision since it may prove as valid to state that red is a result of the mixture of blue and yellow as to state that yellow results from the mixture of red and green.

Summary

A careful survey of the literature on color reveals no adequate experimental determination of the spectral location of the psychologically unique red, yellow, green, and blue. In most cases there has been a failure to distinguish between mixture primaries, invariable hues and psychologically fundamental colors. In a series of experiments, carefully controlled both with respect to the physical stimulus-conditions, and the psychological conditions of observations, we obtained with 10 *O*s the spectral value for yellow 582 mμ, for green 515 mμ, and for blue 476 mμ.[52] All but one (red) of the seven reference standards which are necessary for the psychological description of all colors are now adequately specified.

[50] Judd, Hues of the spectrum colors, *op. cit.*

[51] J. H. Parsons, *An Introduction to the Study of Colour Vision*, 2nd. ed., 1924, 38 ff. A. C. Hardy, *et al., Handbook of Colorimetry*, 1936, 30 f.

[52] Red must be obtained by mixing blue with spectral red. Its determination requires a different apparatus and will be reported in a subsequent paper.

THE INFLUENCE OF PAST EXPERIENCE UPON PERCEPTUAL PROPERTIES

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'Past experience' was for a long time an extremely popular explanatory concept in psychology. Hardly any of the major problems managed to keep from being slurred over and trivialized by reference to past experience. Even in perception there seemed to be no limit to its efficacy. Sensory organization, the constancies, apparent movement—they and many others —were at times attributed to the magic of learning. Then the reaction set in. Except for the field of learning proper, 'past experience' came to be almost synonymous with something whose possibility a decent experimenter should exclude at the very outset. That was a gain, for as long as 'past experience' held sway no other explanation seemed to have a chance. I think we are now free enough to turn around and ask: what, after all, is the effect of past experience upon present experience? More precisely: *how do traces influence the phenomenal appearance of perceived objects?*

Direct influence.[1] First of all, past experience may supply 'meanings' that come to be experienced as intrinsic properties (tertiary qualities) of the things in question, such as the 'sit-on-able-ness' of a chair (Tolman) or the 'man-ness' of the patch in Fig. 1. A sound distinction can be made, however, between *empirical meanings* proper and the underlying perceptual material (including configuration). How, under what conditions, and to what extent, is this realm of 'pure' perception affected by past experience? Again there is one ready answer: as long as the configurational factors inherent in the present stimulation are not too strong, past experience is definitely capable of altering certain *configurational* features of the percept in question. I found the conditions of Fig. 1 well suited to the demonstration of one particular way in which past experience may 'reconfigurate' perception; namely, indirectly, through changing the empirical meaning. Several of the configurational properties in the area on the left of one or the other of the two lines will undergo changes as the area on the right is exposed (for the first time) and the left area thereby is transformed into part of a man—from being nonsensical, or a dog's head, or a whale with a white eye. Some of these changes cannot be at-

* Accepted for publication June 13, 1938.
[1] For the indirect influences see p. 265.

tributed directly to the mere addition of so much configurated area, but must be ascribed to the influence of past experience (here acting through a shift of empirical meaning provoked by the added area). Such changes affect (1) direction or orientation—the dog faces to the left while the same portion of the man is not so polarized; (2) emphasis, shift of focus —which, on purely configurational grounds, should much sooner have

FIG. 1. FIGURE THAT WAS PRESENTED, FIRST PARTIALLY AND THEN AS A WHOLE

shifted to the right than up to the man's head; (3) grouping or segregation—the area of the man's arm is segregated from the area of the trunk, a segregation, or boundary, that had not existed before; (4) depth or stratification—the arm disappears behind the blouse; (5) figure and ground —both on the dog and in the whale the white spot is a figure, focal in the rôle of the whale's eye and peripheral on the dog's muzzle (cf. 2), while in the man it is part of the ground 'seen through.'

Of systematic experiments on other kinds of effect which past experience may have upon present configuration, I wish to name the following: Rubin,[3] working with nonsensical patterns in which either the black or the white area could be seen as 'the figure,' found that patterns which had been learned under the instruction

[3] Edgar Rubin, *Visuell wahrgenommene Figuren*, 1921, 6-21 (Die figurale Nachwirkung).

'white figure,' and patterns which had been learned under the instruction 'black figure,' were, in a test series containing both types of patterns, spontaneously organized the way they had been learned. Gottschaldt,[3] following up a suggestion of Wertheimer,[4] found that as many as 500 repetitions of a simple figure would not help it to be recognized when given as an integral part of a strong configuration. On the other hand, in a series where two simple figures were exposed in a regular rhythm the one that was 'expected' at a given moment would win out over the other when both were exposed simultaneously in a joint pattern with liminal intensities. In a somewhat rash conclusion Gottschaldt postulated that past experience could modify configurations only through giving rise to overt expectations ('specific vectors'). Braly[5] found a perseverative dominance of a learned type of geometrical figures, which were made out of dots and were tachistoscopically exposed. Leeper[6] found, among other things, that previous experience with one of the two aspects of an ambiguous picture made that particular aspect more dominant in the joint picture. Djang,[7] applying Gottschaldt's part-recognition method to relatively weak configurations, found that under such conditions distinctly positive results could be obtained.

May it be emphasized that only in the cases of Rubin and Djang have we clear cut proofs of organization without a vector of expectation. That such is possible is also proved by Fig. 1, as well as by an observation of my own; whenever I have read music for a while all minor inhomogeneities of my visual field look like musical symbols although my 'mind' has already turned away from all concern with music.

After all this it can no longer be doubted that, under somewhat ambiguous conditions, past experience does have a voice in matters of organization. The next question to ask is: can past experience also influence the sensory *material* (the 'stuff'), can it induce *qualities* such as color, taste, or such qualities as 'spatialness,' 'movingness'?[8]

Among the causal factors involved in brightness and color constancy Hering mentions "memory color" to which psychological factor however

[3] Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren, I, *Psychol. Forsch.*, 8, 1926, 261-318; II, *ibid.*, 12, 1929, 1-87.

[4] Max Wertheimer, Untersuchungen zur Lehre von der Gestalt, *ibid.*, 4, 1923, 301-350.

[5] K. W. Braly, The influence of past experience in visual perception, *J. Exper. Psychol.*, 16, 1933, 613-643.

[6] Robert Leeper, A study of a neglected portion of the field of learning—the development of sensory organization, *Ped. Sem.*, 46, 1935, 41-73.

[7] Siao-Sung Djang, The rôle of past experience in the visual apprehension of masked forms, *J. Exper. Psychol.*, 20, 1937, 29-59.

[8] As to the latter type of quality it has been claimed by the so-called 'empiricists' that past experience is quite fundamental in certain fields. How else, so it was argued, but through some associative communication with visual space and visual movement could, for example, a series of kinesthetic sensations ever have gained the qualities of 'spatialness' and 'movingness'? This hypothetical influence of past experience need, by the way, not be due to a point-to-point association, but could be due to a total reinterpretation of one system of differences in terms of the general (spatial) quality of the other (visual) system. Unfortunately, owing to the generality of this alleged effect of past experience, no one has as yet succeeded in devising an experiment which would prove that learning was involved.

he assigns but a minor rôle compared with that of the peripheral adaptive mechanisms.[9] Katz agrees with Hering as to the relative unimportance of memory color: "Thus I cannot assign any great importance to memory colour in connection either with our experiments or with colour constancy in general."[10] However to study memory color in its own right, to investigate its conditions and to assess its potency, lay outside their field of interest (for reasons mentioned above, p. 255). Moreover it was regarded to be "impossible, or at least very difficult, to arrange quantitative experiments with memory colours, on account of their peculiar lability."[11] True,

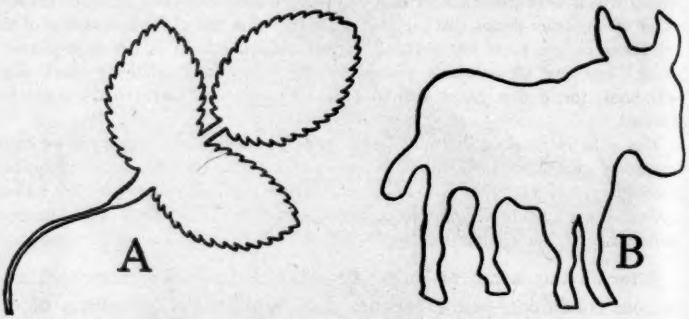

FIG. 2. FIGURES USED IN THE EXPERIMENTS ON MEMORY COLOR
(A is a milliner's green leaf; B is a donkey cut from the same green leaves and patched together.)

memory color is unstable, its influence seems, more so than that of other factors, to require an 'objective attitude,' *i.e.* attending to the colored object instead of attending merely to the color phenomena themselves; it is true also that it took poor illumination for Matthaei to see in a photograph the chromatic colors of the objects represented[12]—but all this does not mean that memory color is any the less worth investigating. Being interested in the effect of past experience upon sensory material we saw no reason for not subjecting memory color to a special experimental treatment.

Experiments on memory color. To experiment on memory color it was necessary to work for just the reverse of what the classical experimenters had been so anxious to obtain. We had to devise a setting that would

[9] Ewald Hering, *Grundzüge der Lehre vom Lichtsinn*, 1920, 6-20.
[10] David Katz, *The World of Colour*, 1935, 265.
[11] *Ibid.*, 162.
[12] R. Matthaei, Die Welt der Farbe, 1927, 310 (Cited in Katz, *op. cit.*, 162).

eliminate all other reasons for one object to show more color constancy than another, except memory color.

Materials. To this end we got some artificial green leaves (such as are used for trimming hats), one leaf was retained as a 'leaf' (cf. Fig. 2A), as convincingly a green leaf as one might desire, and out of some of the others we cut and patched together the picture of a 'donkey' (cf. Fig. 2B), equal to the leaf in all respects but shape,[13] *i.e.* in color, texture,[14] and size.[15] These two green objects, pasted on white paper, were (alternatively, not simultaneously) exposed to such red illumination

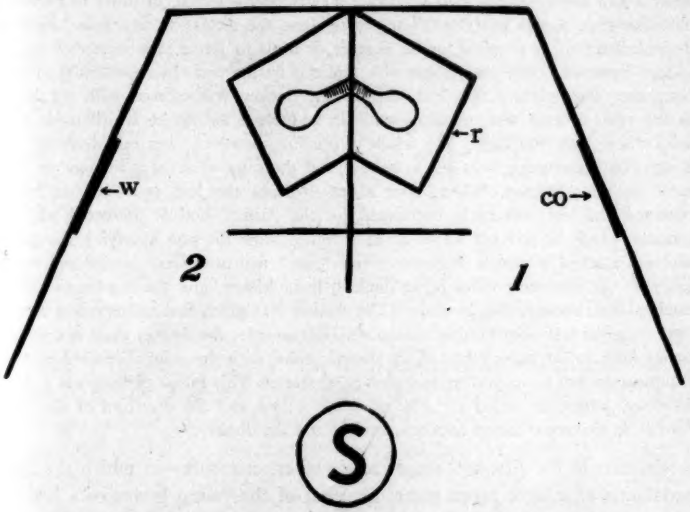

FIG. 3. DIAGRAM OF THE APPARATUS USED IN THE EXPERIMENTS ON MEMORY COLOR

(S = subject; co = critical object; w = color-wheel; r = red glass.)

as would allow them to display but little of their greenness. (If replaced by a circular figure of the same make, the latter would look gray, with only a bare touch of greenness.) Now, if memory color is a factor at all, the leaf, known as green, should appear greener than the donkey, known as gray.

Apparatus. Fig. 3 shows the set-up. Owing to a plate of red glass in front of its lamp, Chamber 1 was under a red illumination which was homogeneous in the area of the critical object and then, spreading out over the white background, gradu-

[13] As for difference in shape see below p. 262.

[14] Figs. 2A and 2B give no more than the outlines of the objects used which in addition showed the veins of leaves.

[15] The difference in size between 2A and 2B was reversed in direction between the experiments of Table I and those of Table II.

ally faded away into shadow. In Chamber 2 the light was normal and, corresponding to the place of the critical object in Chamber 1, there was a color-wheel which *S* had to match with the object by looking back and forth and telling the experimenter in which direction it should be changed (lighter, browner, greener). On the wheel 4 different sectors could be exposed: black, white, a light green, and a yellowish orange.

The procedure was as follows. The 'donkey' was exposed in Chamber 1 while on the color-wheel in Chamber 2 some mixture in the phenomenal vicinity of the crucial object was offered, *e.g.* 300° black, 20° white, 20° orange, and 20° green. After *S* had expressed his wish the wheel was stopped—which, in order to prevent objective cues, *S* was not allowed to watch—and the sectors were changed in the desired direction. It required, on an average, 6 trials to arrive at a somewhat satisfactory equation. Then the donkey was replaced by the leaf. In about 50% of all cases the same mixture that had satisfied the donkey was offered with the leaf, in the other cases *S* was presented with an additional 20° white which made the color-wheel look too light. The whole procedure, however, was not disclosed to *S* who, on the contrary, was led to believe that the color-wheel was changed in the most irregular fashion. When, after about 6 trials, the leaf equation had been obtained, the leaf was again exchanged for the donkey, and *S*, presented with a mixture which he did not know to be identical with the one satisfying the leaf, was again asked to match. Between 3 and 5 such matching were carried out with every *S*, the minimum series being donkey, leaf, donkey, and the maximum being donkey, leaf, donkey, leaf, donkey. (The donkey was given first to prevent a carry-over from the leaf—for the leaf was more liable to color the donkey than vice versa, owing both to the coincidence of its memory color with the color disguised by the illumination and to its reality, non-picture, character. This factor of temporal order, however, cannot be called upon to explain the fact and the direction of the difference in greenness found between the leaf and the donkey.)

Results. In the first and major series of experiments—in which the material was of a light green color, like that of the young leaves of a beech-tree, and the donkey was a little smaller than the leaf—11 *S*s took part in the experiments. For 6 of them the leaf appeared decidedly and measurably greener than the donkey (cf. Table I). For 2 *S*s the leaf tended to look a little greener, but no equation could be set up. For 3 *S*s there was no difference at all. It is interesting that of these 'negativists,' one was a painter (a painter is used to abstracting from objective connotations), another was a psychologist who knew the problem and moreover was "distinctly uninterested in and poor on colors," and the third was a medical student, also more sophisticated than the majority of the *S*s. There was no case of reversal, *i.e.* of the donkey being greener than the leaf.

Table I presents in degrees the angles of the green (G), orange (O), and white (W) sectors which were found equivalent to each of the two critical objects.[16] Every figure represents the average of from 1 to 4 values,

[16] The remaining portions of the color-wheel were black.

each being the outcome of about 6 trials. This table shows that these 6 *Ss* were just about agreed as to the magnitude of the difference. The mixture which was equivalent to the leaf contained on an average 100% more green and 100% more orange than the one equivalent to the donkey. But what does this difference mean in phenomenal terms? The numerical

TABLE I

MIXTURES ON THE COLOR WHEEL (IN DEGREES) THAT WERE EQUIVALENT TO THE COLOR OF THE DONKEY AND THE LEAF

(G = green; O = orange; W = white. Remaining portions of the wheel were black.)

S	Donkey				Leaf			
	G	O	W	No.	G	O	W	No.
1	30±5	30±5	25±5	2	55	25	15	1
2	26±3	7±4	15±3	4	58	21	0	1
3	33±9	13±4	110±13	3	50±10	18±8	95±5	2
4	29±1	15±2	24±1	2	63±2	63±2	0±0	2
5	25±0	11±1	13±3	2	55	50	0	1
6	33±3	38±3	10±10	2	80	40	0	1
Av.	29	19	33		60	36	18	

differences certainly give impression of a much greater difference than was actually seen. Actually they correspond to a phenomenal difference which is *little more than just distinctly noticeable* (when the two equivalents are shown side by side). The phenomenal mean—as to greenness—between the leaf-equivalent and a pure gray of equal brightness was found to be somewhere in the close vicinity of 25°G, 13°O and 33°W, that is to say, the donkey's equivalent is just about half as green as the leaf's equivalent.

TABLE II

MIXTURES ON THE COLOR WHEEL (IN DEGREES) THAT WERE EQUIVALENT TO THE MODIFIED STIMULUS-OBJECTS

(G = green; O = orange; W = white. Remaining portions were black.)

S	Donkey				Leaf			
	G	O	W	No.	G	O	W	No.
1	29±1	10±1	0±0	2	79±6	27±1	0±0	2
2	31±4	6±1	0±0	3	59±0	16±3	4±4	2
3	26±1	7±0	11±2	2	42±4	10±1	3±3	2
Av.	29	8	4		60	18	2	

In a second series of experiments the material was of a darker green, like that of full-grown leaves. The donkey was this time a little larger than the leaf. There were 4 *Ss*. With 3 of the *Ss*, the leaf was decidedly and measurably greener than the donkey. For 1 *S* there was no difference. Table II gives the results for the 3 *Ss*. As will have been noticed Table II

is in perfect accordance with Table I, despite the differences in shade and size.

Besides memory color the only differences between donkey and leaf which could possibly cause phenomenal changes are the difference in *shape* and the difference between a *picture* and a real object. Why, however, should the shape of a three-divisioned leaf look greener, or show more color constancy, than the shape of a quadruped animal? True, the donkey was a little more dispersed than the leaf, but control observations showed that this factor cannot be regarded as a rival of memory color. As to picture versus reality, why should real objects look greener, or show more color constancy, than pictures when made of the very same material? In any case control observations proved that the leaf also looked greener than a simple figure that had no picture character, *e.g.* a circle.

We are then safe in stating that under our conditions memory color exerted a noticeable and measurable influence upon the color of objects exposed under adverse illumination. Traces may color perceptual objects. It remains to specify the conditions.

Four features proved to be relevant: (1) The two objects should not be exposed simultaneously side by side. There is that peculiar lability of color phenomena under adverse illumination which is liable to rule out a difference as soon as strong factors favoring equality are brought in. This tendency towards *'assimilation'* is not a function merely of the amount of difference, for we found that the two equivalents, though having by definition the same difference, were much less endangered by assimilative tendencies. They held their own even when exposed side by side. (2) The illumination should not be too one-sided, *i.e.* too *monochromatic*. For an effect of memory color to show itself as markedly as it did in our experiments we found it necessary for the memory color to have some degree of sensory support, *i.e.* to increase a given shade rather than to create one that is not represented at all on the stimulus side. (3) In our setting, the awareness of a separate and *abnormal* illumination was not itself a function of memory color—as it would have been had it been left to the leaf to indicate to *S* that something must be 'wrong' with the illumination. There are cases of this sort, however, and it is here that a more drastic effect of memory color may occasionally be observed: a sudden switch-over to a different illumination with correspondingly different surface colors. (4) *S* should not know the issue and thereby be led to adopt an unduly critical or abstract *attitude*. As stated above, the influence of no other factor involved in color constancy is so liable to be impaired by a 'subjective' attitude *i.e.* abstracting from the colored object, as is memory

color. It is obvious that an effect issuing from the object as such must require a relatively 'objective' (concrete) attitude. What was not predictable, however, certainly not on the basis of Katz' statement,[17] is the fact that memory color proved tenacious enough to maintain itself in spite of an attitude of comparing and matching colors which certainly is itself a pretty abstract and artificial one. This means at the same time that the difference between the equivalents represents only the lower limit of what difference might be experienced with a more natural attitude.

Experiments on memory taste. The following experiments were carried out to show whether, how, and to what extent, memory taste may affect actual taste.

Procedure. To arouse customers' buying curiosity, I take it, some firms recently placed upon the market 'white chocolate,' a cream-colored substance in which only the oil of the chocolate fruit is used and which tastes like milk chocolate but for a slight reduction of flavor (at least in the kind we used). To find out whether the milkish or creamlike visual appearance of this kind of sweet would suggest a corresponding element of taste appearance, *S* tasted, in succession: (1) a piece of ordinary brown milk chocolate, while blindfolded; (2) a piece of white chocolate, while blind-folded; (3) as (1) but with eyes open; and (4) as (2), with eyes open. If the influence we are looking for existed, (4) should taste more milky, or less chocolate, or at least more different from the rest, than (2). After each experience, *S* was asked to describe it, and, from (2) onwards, to compare it with the preceding experiences. Between two successive trials he was asked to drink some water to cleanse his mouth from the previous taste. The transition from the blindfolded to the seeing state was made to seem like an unintentional occurrence. While in (1) and (2) strict care was taken that *S* had his eyes closed before the chocolate was taken out of the bag, when it came to (3) this precaution was not only dropped but in the pause preceding it the experimenter, as though in preparation for the next trial and while casually conversing, handled the next piece in front of *S* until he was sure that it had been noticed; if *S* remarked that, unfortunately, he had got a glimpse of it, the experimenter would say: "Oh, that does not really matter." The same happened before presentation of (4) with still less pretended caution.

Results. Seven subjects took part in the experiment. In four cases the fourth piece was reported to taste milkier than the rest, including the second ("too buttery," "taste of condensed milk," "like creamtoffee," twice) in one case it was said to have "no chocolate taste, less than the rest," and in another case it had "less taste than any." Only in one case there was no difference, but this subject (Professor Katz) was also the only one who knew of white chocolate. He had at once become suspicious, especially when the second piece was given, that white chocolate was in the game. This case is then to be subtracted, leaving *100%* (more or less)

[17] Katz, *op. cit.*, 162.

positive cases, of which ⅔ were in terms of milkishness and ⅓ in terms of 'non-chocolate' or 'different.'

In four more experiments, in order to improve the experimental technique, we changed the instruction to the effect that *S* was explicitly asked to compare (2) with (1) and (4) with (3) and to compare the respective differences. To our sincere surprise there was *no effect* of memory taste in any of these additional four cases. Gradually the reason dawned upon us and was then amply verified by pertinent remarks of the *Ss*. The instruction to compare (2) with (1), and (4) with (3), had managed to arouse in all four *Ss* a suspicion that objectively (3) and (4) were identical with (1) and (2). This suspicion arose at the latest when the blindfolding was abandoned after (2), which, although it had not been done any more conspicuously than in the first experimental series, now coincided with the hiatus between the two pairs and therefore no longer passed as harmless chance. We do not venture to say that a very keen and trained introspectionist could not have resisted falling for the stimulus-error, but, however that may be, we do think that the suspicion was not the only factor reducing the effect of memory taste. The very grouping in the rhythm 1 2′, 3 4′, with its configurational bias towards assimilation between analogous members of the two pairs could not but help to reduce the effect (compared with the earlier arrangements where the fourth piece had a more homogeneous background from which to stand out). At any rate it is in itself an interesting fact that so minute a difference in the procedure could overthrow the memory effect. This points to the highly complex nature of memory taste and is in good accordance with what was found in the case of memory color (cf. the overthrow of the effect when the two objects were exposed simultaneously side by side).

Conclusion. The experiments reported in the two preceding sections (on memory color and memory taste) have shown that there are conditions under which past experience (some trace-system) affects sensory material, no search (or vector) being involved. These conditions were found to be analogous to those applying to sensory organization (configuration proper): (1) weak opposition from, or, better still, some support from present stimulation; (2) a perceptual attitude that, instead of 'skimming off' (and thereby actually creating) 'sense-data,' aims at 'the true' properties of objects known or expected. Memory qualities thus represent the most elementary, still quasi-sensory, way in which knowledge may 'transcend' immediate stimulation.

All those types of learning-effect so far discussed belong to a special

class which should be well marked off from another class containing instances of *indirect* influence of past experience. There is a great variety of cases in which past experience comes in but does no more than supply conditions for other factors to work upon. It sets, as it were, the stage for the dynamics of forces which in themselves have nothing to do with past experience or learning (at least not with the learning that set the stage). Only recently this sort of indirect influence has been explicitly acknowledged within the field of perception.

In experiments on the effect of past experience upon induced movement Krolik found that a perceived picture whose object is known as occurring within some typical setting would tend to reproduce this setting and thereby affect other perceived objects almost as if that setting too had been actually perceived.[18] Thus the picture of a lighthouse which was objectively moved in relation to the picture of a ship would, by calling up the whole maritime scene, induce the ship to move—according to the general law that of two objects in relative movement one of which forms the environment or 'frame of reference' of the other, the latter will tend to take over the movement.[19] That is to say, it is not the movingness, but the surroundedness, of the ship that is 'copied' from past experience.

Among the stimulus-objects used by Mikesell and Bentley, there were 'conventional' ones, *e.g.* undistorted letters, in which past experience made for a more unitary configuration which then, in its turn, affected contrast, through laws that apply to unitary configurations (and contrast) as such, no matter how they have arisen.[20] (This shows that convention may work *through* configuration.) A similar sort of thing may occur in all constancies, because here, essentially, two variables are involved, an object and a medium, which are so interrelated that if past experience affects the one it cannot but indirectly affect the other too. Thus an empirical cue for size must affect distance and an empirical cue for distance must affect size. (In our experiments on memory color we had a direct influence since the illumination was already fully defined by other cues.)

There are cases, closely akin to that of Krolik, where past experience does nothing but determine the *level*, the *norm*, by deviation from which an object then acquires new properties. For example a noise is 'loud' when preceded by low noises. These cases are of great importance in the field of tertiary qualities. An unusual costume looks 'odd,' an unusually scanty (or rich) costume may look 'indecent' (or 'pompous'), not because, properly speaking, habit determines indecency or pompousness, but because habit determines the level, a deviation, in a certain direction, from which is the adequate stimulus for the quality 'indecent,' or 'pompous.' Anthropologists, in stressing the rôle of custom, have been inclined to overlook the indirectness of much of its influence.

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[18] Walter Krolik, Über Erfahrungwirkungen beim Bewegungssehen, *Psychol. Forsch.*, 20, 1935, 47-101.

[19] This law is not itself a product of past experience because it also applies to objects not known as moving.

[20] W. H. Mikesell and Madison Bentley, Configurations and brightness contrast, *J. Exper. Psychol.*, 13, 1930, 1-23. Cf. also Wolfgang Metzger's discussion Gestalt und Kontrast, *Psychol. Forsch.*, 15, 1931, 374-386, esp. 382.

DIFFERENTIAL EFFECTS OF CURARE UPON HIGHER AND LOWER LEVELS OF THE CENTRAL NERVOUS SYSTEM

By ELMER CULLER, J. D. COAKLEY, P. S. SHURRAGER, University of Rochester, and H. W. ADES, Johns Hopkins University

In an earlier article from this Laboratory,[1] Girden and Culler demonstrated that a conditioned pattern could be acquired, retained and revived by curarized dogs. The animals are prepared as follows. Both semitendinosus muscles are exposed; either one contracts sharply when the homolateral hind paw is duly stimulated (electric shock). The right member of the pair is first conditioned until it replies promptly and vigorously to the sound of a bell. The animal is then curarized intra venam; whereupon we find that, even though this right muscle is still capable of a slight but clear twitch when the paw is stimulated, the CR (conditioned reaction) which was previously manifest has utterly vanished. The left semi-tendinosus is now conditioned under curare to the same sound (shock to left paw), until it twitches every time the bell is rung. Upon recovery from the drug (three or more hours later), the dog is again tested with bell; the right muscle's *normal* CR reappears clear and strong, whereas the curare-CR in left muscle betrays no sign of its presence. Upon recurarization, bilateral reversal once more occurs; the normal CR on right side wholly vanishes while the curare-CR on the left side again emerges. The facts schematize as follows:

Condition	Semi-tendinosus muscles	
	Right	Left
Normal	CR established	
Curare	CR vanishes	CR established
Normal	CR reappears	CR vanishes
Curare	CR vanishes	CR reappears

These results carry a real theoretic challenge. We see that a dog can be independently conditioned on separate levels or by disparate patterns

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[1] E. Girden and E. Culler, Conditioned responses in curarized striate muscles in dogs. *J. Comp. Psychol.*, 23, 1937, 261-274.

of the central nervous system to the same stimulus at the same time. It may well be called an experimental form of *dual personality:* the animal replies by two independent behavior-systems to the same stimulus, one in one state and one in the other. Normal learning proceeds at cortical levels, curarized learning at other (presumably sub-cortical) levels.

The following mechanism was hypothetized to account for these phenomena. The animal under curare is functionally decorticate (cortex depressed); what conditioning occurs is, therefore, sub-cortical in nature and locus, being inhibited when the cortex returns to dominance. Conditioning of the normal animal however is predominantly cortical in nature and locus, and therefore disappears when curare depresses cortical activity.[2]

In this paper we seek to test the validity of this inferential mechanism with evidence drawn from direct stimulation of motor roots and of cortex.

Procedure. Under complete general anesthesia (ether), the trachea is cannulated and the right M. semi-tendinosus dissected out. After its distal end has been freed, the blood and nerve-supply being kept intact, the muscle is available for ready observation and manipulation. Several segments of the cord (sixth and seventh lumbar, first sacral) are so exposed that the ventral roots which innervate M. semi-tendinosus can be fully isolated and directly stimulated with suitable electrodes. The left motor cortex finally is laid bare around the cruciate sulcus. With the animal thus prepared, the head is snugly secured in a holder, the fore and hind limbs being so disposed as to be comfortable for the animal and convenient for examination by the observer. Stimulation is provided by a rectangular pulse of variable amplitude and duration, generated in a chronaximeter with a pentode-circuit so designed as to maintain a horizontal current of uniform amplitude despite large and sudden changes in tissue-resistance. This latter feature is of utmost value and import, since the stimulus remains true despite unavoidable and unknown changes in moisture, ionization and polarization. Unipolar electrodes, with diffuse positive lead to head-holder or to the animal's exposed skin, are used; a silver bulb, less than 1 mm. in diam., or in lieu thereof an acuate wick soaked in blood serum or saline, on the cortex; on the cord a silver hook, fitted to disengage the motor root gently from body-fluids and hold it free of adjacent tissue. With both cortex and motor-root in each animal, a spot was first located which seemed to be optimal for stimulation; that is, for eliciting a perceptible contraction of M. semi-tendinosus with lower current-density (during 500 ms.) than any other. This point was duly noted and used throughout all later tests. During stimulation every care was employed to keep each spot warm (with the lamp which supplied both light and heat) and moist (with physiological saline or blood serum); but, to obviate dissipation of the stimulating current, each spot was gently and effectively dried just prior to applying the electrode. Indeed, a special assistant stood by as needed, to mop the ventral root with cotton pledgets. Two normal limens were regularly secured for cortex and spinal root; if these two differed by more than 5 to 8%, stimulation was

[2] *Ibid.*, 273 f.

continued with added precaution until figures within that range were achieved. Thereupon curare was cautiously introduced (through V. saphena) until standard symptoms appeared: respiratory paralysis, depression of corneal lid-reflex, muscular flaccidity and inertness under stimulation. Mechanical ventilation was in all cases required. After curare-limens had been determined for both cord and cortex once or preferably twice, the animal was given time to recover, insufflation being continued until every trace of respiratory blockage had vanished. After not less than three hours, final control tests were made; no data are included without these check-readings from the post-curare normal state.

Excluding operative fatalities, no less than 36 dogs were used; but our protocols for over half of them were too meager for use. Curare is at best a tricky instrument, never pure, rarely uniform. A good many succumbed before final recovery from the drug; in other cases it was virtually impossible for obscure reasons (some independent variable or some imperceptible lesion?) to determine what the rheobase truly was. Despite this wastage, the 12 dogs listed below contributed each one or two complete sets of data: a 'set' consisting of six independent measures, one each before, during and after curare for both cortex and ventral roots. It is the records from these dogs which are here reproduced and analyzed.

Limens, whether of current or of duration, were measured in the customary manner. Of three collaborators, one would manipulate the negative electrode, guarding the tissue as above explained; a second would prepare optimal conditions for observing, under controlled illumination, the exposed M. semi-tendinosus, whose response was the ultimate criterion for all thresholds. Since but a few fibers contract under liminal stimulation, the locus and pattern of this critical response had to be studied and noted with care for use in all later tests on that animal.[3] A third collaborator operated the chronaximeter, first setting the current amply high for a generous response, then attenuating it in small stated steps until the muscle fell from 100% through 50% to 0% in its replies; then starting below zero, he raised the pulse-amplitude until the muscle-response again appeared each time. The mean of the two 50%-points was taken as the 'limen.'

Results. The records divide properly into two groups: (a) the *time,* whether in milliseconds (cortex) or micro-seconds (ventral roots), required for a current whose magnitude equals twice the normal rheobase, to evoke the critical (just perceptible) response of M. semi-tendinosus; (b) the *current,* whether in milliamperes (cortex) or micro-amperes (ventral roots), required to elicit the same criterion. In all cases, we first measure the liminal current (rheobase) for indefinite time (500 ms.); this current is then doubled and the liminal duration (chronaxy) measured. But under curare, though the rheobase has changed, we keep on using the same current-amplitude as in the normal state. In this paper we are not concerned with the chronaxy as such; rather with the *relative durations* of the same current in the two states. This duration happens to

[3] The normal locus and pattern of response may change in some degree under curare.

be true chronaxy in the normal state but not so in the other; hence the term, with all its controversial implications, is carefully shunned throughout this paper.

TABLE I

MINIMAL TIME REQUIRED TO EVOKE CRITICAL RESPONSE OF RIGHT M. SEMI-TENDINOSUS UNDER NORMAL AND CURARIZED CONDITIONS

Dog	Cortex (ms.)			Motor roots (mms.)		
	normal	curare	normal	normal	curare	normal
1	5.70	10.90	5.70	67	74	67
2a	5.30	5.30	5.30	76	53	53
2b	5.30	6.40	4.70	53	76	59
3a	3.20	4.00	4.00	112	89	89
3b	4.00	5.50	4.00	89	167	89
4	5.40	4.80	2.50	114	75	83
6	6.70	6.40	7.10	71	75	67
7	6.70	5.65	5.40	68	110	73
8	5.50	9.10	5.50	71	40	89
9	5.00	5.50	5.50	72	45	76
10	5.30	4.20	2.60	93	77	138
11a	5.50	7.80	4.80	68	110	88
11b	4.80	5.50	4.50	88	58	75
12	6.60	7.80	4.95	144	81	64
14	5.20	5.20	4.60	260	105	245
(1) Sums	80.20	94.05	71.15	1446	1235	1355
(2) Means	5.347	6.270	4.743	96.4	82.3	90.3
(3) Mean-diff.	0.923	1.527		14.1	8.0	
(4) S. E. $M_{diff.}$	.445	.444		13.9	12.9	
(5) Values of t	2.074	3.432		1.014	0.620	
(6) Values of P	.029	.002		.167	.273	
(7) Values of $-\log_e P$	3.5405	6.2146		1.7898	1.2983	

In Table I are mustered all the cases for which complete records from both cortex and motor root before, during and after curare were derived: 9 dogs with one complete set each and 3 with two sets each, a total of 15 acceptable records. Considering the cortex, we see that liminal duration for twice the rheobase rises from 5.347 ms. in the normal state to 6.270 ms. under curare and then again falls to 4.743 ms. as normalcy returns. Under curare the liminal stimulus-duration lengthens by 0.923 ms., after curare it again shortens by 1.527 ms.; the mean change being 1.225 ms. The values of t (critical ratio) for these magnitudes are, in order, 2.074, 3.432 and 3.000. In a system with 14 degrees of freedom,[4] the last two of these are dependably 'significant,' there being well less than one chance in a hundred of drawing so great a change by random sampling of a homogeneous distribution. The first change ($t = 2.07$) is probably, though not surely, dependable. Considering the three figures, however, we can state with assur-

[4] R. A. Fisher, *Statistical Methods for Research Workers*, 5th ed., 1934, chap. 5.

ance that, given a constant amperage (ordinate), the duration (abscissa) and therefore the quantity (area) of electric energy must be reliably augmented under curare to evoke a critical response of the S-T muscle.

In the motor roots, the picture is indeed different. The mean excitation-time falls by 14.07 mms. (micro-seconds) under curare, rising again by 8.00 mms. post curare; the mean curare-effect being 11.03 mms. The critical ratios are, in order, 1.014, 0.620 and 0.853. The mean stimulation-time under curare is lower than either before or after, but the change is not dependably greater than zero. The sole alternative conclusions are, that excitation-time for the ventral roots under curare is *either* the same as, *or* shorter than, in the normal state. We can thus assert with fullest assurance that stimulation-time under curare does *not* lengthen at the motor roots as it does in the cortex.

TABLE II

MINIMAL CURRENT REQUIRED TO EVOKE CRITICAL RESPONSE OF RIGHT M. SEMI-TENDINOSUS UNDER NORMAL AND CURARIZED CONDITIONS

Dog	Cortex (ma.)			Motor roots (mma.)		
	normal	curare	normal	normal	curare	normal
1	1.875	2.250	1.875	0.200	0.150	0.250
2	1.375	1.375	1.125	0.475	0.350	0.450
3	0.850	0.950	0.775	0.700	0.450	0.750
4	0.800	0.950	0.750	0.200	0.100	0.200
(1) Sums	4.900	5.525	4.525	1.575	1.050	1.650
(2) Means	1.225	1.381	1.131	0.394	0.263	0.413
(3) Mean diff.	0.156	0.250		0.131	0.150	
(4) S. E. $M_{diff.}$	.0793	.0445		.0425	.0500	
(5) Values of t	1.967	5.618		3.082	3.000	
(6) Values of P	.0721	.0056		.0270	.0288	
(7) Values of $-\log_e P$	2.6297	5.1850		3.6119	3.5474	

In four dogs (see Table II) we compared the minimal current instead of minimal time. We see that, when time is fixed at 500 ms., the current must be increased; just as, when current was fixed at twice the rheobase, duration had to be increased. The critical ratios (t) for cortex are 1.967 and 5.618, for motor roots are 3.082 and 3.000. With but three degrees of freedom, only 5.618 is comfortably 'significant.' The mean difference between curare and control is 0.203 (cortex) and 0.141 (motor roots); their critical ratios are 3.484 and 3.097. Here again we may infer that curare increases the quanta (current $\times$ duration) of electric energy required to stimulate, while it probably, but not surely, sensitizes the motor roots. Since $Q = i \cdot t$ (quantity of electric energy equals current times duration), we see that curare depresses cortical function, whether measured by duration or current and does *not* depress motor roots by either criterion. The statistical analysis may

with profit be carried further. Even though some of the curare-changes are not separately dependable, their complete concurrence (all the cortical limens, whether of time or current, being elevated and all the peripheral limens being depressed) strongly suggests that their combined significance is far greater. When each item reënforces the others, the aggregate is more significant than is any of its parts; even as a mean is more stable than are the component measures. By Fisher's method for estimating the significance of an aggregate,[5] we determine (1) the chance-frequency, P, of each item and (2) the value of $-\log_e P$ for this chance-frequency. Twice the sum of these values in (2) equals X^2 for that particular combination of items, whether pre- or post-curare changes, in cortex or motor roots, for excitation-time or current. We can thus estimate what degree of confidence may safely be attached to each degree of complexity from the single item to the whole system.

In the subjoined tabulation, 'pre' is before and 'post' is after curare; 'time' is liminal excitation-time and 'current' is liminal value of direct-current pulse; 'C' is cortex and 'M' is motor-root; 'P' is the frequency with which a given combination would occur in random samples from a population which is really homogeneous. All estimates of 'P' are taken from "Student's" table with due regard to degrees of freedom.[6] When $P = 0.010$ or less, the result may with full confidence be accepted as significant.

Interpretation and summary. This study has no concern with technical problems of chronaxy, such as appear in the many reports of Lapicque, Monnier, Rushton and others.[7] The questions and dubieties which beset this topic need not delay us here. We have the modest but definite task of interpreting the above material in conjunction with the earlier data of Girden and Culler on conditioning in the curarized dog.

(1) We can say, with full assurance ($P < .010$; cf. Table III, A 1 and B 1) that curare elevates *both* the cortical rheobase, i (excitation-time being fixed at 500 ms.) *and* the cortical excitation-time, t (current being fixed at twice the animal's normal rheobase). In either case, the product $i \cdot t$ indicates the number of electric quanta which must be delivered to the cortical motor-point ere a response of contralateral M. semi-tendinosus can just be detected. By each criterion, more energy is needed to excite the neuro-motor mechanism. The depressant action of curare upon the higher levels of a typical motor response thus seems to be clearly demonstrated.

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[5] *Op. cit.*

[6] Student: New tables for testing the significance of observations, *Metron*, 5, 1925, 105-108.

[7] H. Davis and A. Forbes, Chronaxie, *Physiol. Rev.*, 18, 1936, 407-441.

(2) The same response of M. semi-tendinosus, when initiated at the ventral root, shows no signs of depression. The limens for both excitation-time and current are probably, but not dependably, reduced (cf. Table III, A 2, B 2, C 6); but in any case one can say with certainty that they are *not* elevated, as in the cortex. It is notable that transmission over the neuro-myal junction is fully as efficient for certain elements of the excitatory mechanism as under normal conditions. We see again the "hobble-effect" previously described;[8] that whereas a complete response is abolished by curare, the minimal response is not likewise affected; the muscle proves to be, if anything, more responsive in some of its fibers to liminal stimulation than ever. Whereas some fibers are effectively blocked, other paths are still kept

TABLE III

SUMMARY OF RESULTS

A. Time	P	B. Current	P
1. Pre and post, C	<.010	1. Pre and post, C	<.010
2. Pre and post, M	.189	2. Pre and post, M	<.010
3. Pre, C and M	.031	3. Pre, C and M	.014
4. Post, C and M	<.010	4. Post, C and M	<.010
5. Pre and post, C and M	<.010	5. Pre and post, C and M	<.010

C. Time and Current	P		P
1. Pre, C	.015	5. Pre and post, C	<.010
2. Post, C	<.010	6. Pre and post, M	<.010
3. Pre, M	.029	7. Pre, C and M	<.010
4. Post, M	.047	8. Post, C and M	<.010
		9. Pre and post, C and M	<.010

open. It is one of the special features of our method that this selective action by the drug is clearly revealed. These twitches, minute in both amplitude and area, are unable to move the entire leg and are thus quite imperceptible when the limb is intact. Further the observer, with optimal placement and illumination of the exposed muscle, can spot the exact locus and distribution of active fibers better, we think, than by electric recording.

(3) The most sensitive fibers, which reply to minimal stimulation in the normal state, will not always retain their primacy under the drug. Instead, others which normally are above the limen will take over the liminal response.[9] This fully demonstrates the selective character of curare-action: some fibers (neural or myal) seem to be depressed; others to be sensitized.

(4) Somewhere between cortex and ventral-root passes a *plane of cleavage:* a surface whose existence is demonstrated by our data even though

[8] Girden and Culler, *op. cit.*
[9] This will be more fully treated in a separate paper.

its location and contour cannot be definitely stated. To one side of the boundary (cephalad) is a zone of depression; to the other (caudad) is a non-depressed (possibly sensitized) area. In the former paper,[10] the boundary was assumed to lie between cortex and thalamus; the curarized animal being still capable of conditioning, Girden and Culler reasoned that the surface of differentiation extended above the diencephalon. Later evidence indicates the possibility of conditioning on still lower levels even down to the spinal cord;[11] hence it is not impossible that the plane of cleavage passes somewhere *below* the thalamus in our animals.[12]

In any case, this evidence in conjunction with the previous report by Girden and Culler, clearly indicates that "dual personality" may be effected by the differential action of curare upon higher and lower levels of the central nervous system. In the present state of knowledge, personality-cleavage is a concept derived wholly from behavioral evidence; for which concept the above data provide a possible physiological mechanism in the neural structures. How this experimental scission of a dog into two distinct behavior-systems by selective action of curare resembles the multiple personalities of human pathology is not obvious. In man the secondary state often appears to be infantile and disinhibited, as though it were relieved (as in our dogs) of cortical dominance; but in practice it seems clear that the plane of cleavage will seldom have the same locus or configuration in any two human cases. In any event, the above material provides some neural confirmation, and rationalization for the evidence that a behavior-system can be temporarily split by the differential action of drugs.

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[10] Girden and Culler, *op. cit.*

[11] P. S. Shurrager and E. A. Culler, Phenomena allied to conditioning in the spinal dog, *Amer. J. Physiol.*, 123, 1938, 186-187.

[12] Since Girden and Culler's animals were conditioned to sound, the plane must in this case pass somewhere *above* the cochlear nuclei in the medulla.

APPARATUS

MEASURING THE STEADINESS OF MANUAL MOVEMENT

By HERBERT GURNEE, Western Reserve University

There has long been need for a test of the steadiness of manual movement, a test that would meet modern standards. Most instruments of this sort have failed to meet the essential requirements of reliability. Thus the familiar tracing board of Whipple was early described as unreliable by Langfeld,[1] and recently the authors of the Minnesota Mechanical Ability Tests[2] have reported for it a test-retest reliability of 0.32 (error scores) and 0.40 (point scores). A paper tracing test has fared almost equally poorly.

The present article describes the results of an effort to develop a satisfactory tracing-movement test. By *satisfactory* is meant a test which possesses high reliability, simplicity, easy administration, portability, and low cost.

The construction of the instrument is as follows.[3] In a brass sheet, 1/32 in. thick, 22 cm. long by 8 cm. wide, a figure is cut, in the shape of an inverse S, which is 19 cm. long (linear measurement) and 4 mm. wide. The sides of the figure are arcs of true circles with radii of 66 mm. inside and 70 mm. outside. Each side of the figure is broken at equal intervals by 9 V-shaped notches, 3 mm. deep and 4 mm. across the mouth. The figure terminates at each end in a hole 1 cm. in diam. This plate is fastened to a piece of 5-ply maple, 26 cm. long, 16 cm. wide and 3/8 in. thick, into which a figure is cut corresponding to that in the metal, but 2 mm. wider and without notches. The wood is cut with an underbevel, so that a stylus will touch it only at the notches and at the ends of the figure. The insulation thus provided at the ends is introduced to prevent contact before *S*'s movement starts and after it is completed. The insulation at the notches serves the purpose of compelling breaks in the circuit if *S* tries to run along the side of the plate. The notches provide a means of controlling the rate of his movement. One unsatisfactory feature of the tracing board has been the impracticability, not to say impossibility, of ensuring uniform rate of movement.

The maple board, with plate attached, is fastened, at an angle of 45° vertically, to a wooden box 22½ cm. high, 19 cm. deep and 16. cm wide. A terminal near the top gives electrical connection with the plate. Rubber-tack bumpers are fastened to the bottom of the box to prevent slipping.

The instrument is placed in series with an electric counter (Stoelting no. 22407), four dry cells and a stylus. The stylus is made by Stoelting for use with the Whipple tracing board and has a 1/16-in. pin.

Because the top of a table proved too high, the instrument was placed on a shelf made by clamping a board to the underside of an ordinary kitchen-size table.

[1] H. S. Langfeld, Voluntary movement under positive and negative instructions, *Psychol. Rev.*, 20, 1913, 459-478.

[2] D. G. Paterson, R. M. Elliot, L. D. Anderson, H. A. Toops, and E. Heidbreder, *The Minnesota Mechanical Ability Tests*, 1930, 87.

[3] The instrument is now obtainable from C. H. Stoelting Co., Chicago.

The top of this shelf was 64 cm. from the floor. *S* sat in a straight-backed chair (about 46 cm. high), directly in front of the instrument and half an arm's length away. A daylight desk-lamp was placed on the table just over the instrument, because in no other way could the production of shadows be eliminated. The room had only one window.

The rate of movement was controlled by a metronome which was set at 100 beats per min. Preliminary experimentation indicated this to be a satisfactory rate. *S*'s instructions were as follows:

"This is a test of steadiness. Insert the pin of the stylus about one-third to one-half of the way into the groove and move without stopping from the upper end to the lower end and back trying to avoid touching the sides. Each time you touch the sides an error will be recorded against you. Move at the rate of one notch to each beat of the metronome. It will help to watch the stylus from the side so as to see where you are going. Also, it will be to your advantage to keep the pointer perpendicular, because, if you slant it, you will be closer to the sides. You will get one practice trial with each hand and then six more trials with each hand."

TABLE I

DECILE NORMS FOR THE AVERAGE NUMBER OF CONTACTS IN SIX TRIALS WITH EACH HAND

Decile	Right hand	Left hand
10	0 - 4.6	0 - 8.4
9	4.7- 5.8	8.5- 9.6
8	5.9- 6.9	9.7-10.8
7	7.0- 8.0	10.9-12.0
6	8.1- 9.0	12.1-13.2
5	9.1-10.0	13.3-14.3
4	10.1-11.0	14.4-15.4
3	11.1-12.0	15.5-17.6
2	12.1-15.0	17.7-19.6
1	15.1 or more	19.7 or more

Appropriate demonstration accompanied the instructions, and the subject was cautioned against supporting his arm in any way. As a matter of fact, use of the instrument requires a freely moving elbow. The trials started with the right hand and alternated left and right thereafter until fourteen trials in all had been completed. A rest period of about 20 secs. intervened between trials, and during this time *S* was told to relax. He was not permitted to smoke or to leave his seat. Meanwhile *E* took the record of the counter. Forty-five male undergraduates, chosen at random from an elementary course, were used as *Ss*.

In the treatment of results, the score was determined by dropping the first record for each hand and averaging the remaining six records. The higher the score, the poorer was the performance. The reliability of the instrument was determined by correlating the averages of the three odd trials against the averages of the three even trials. The resulting coefficients were 0.818 for the right hand and 0.765 for the left hand. These values do not quite reach the point, 0.85, generally accepted as the minimum of adequate reliability. By applying the Spearman-Brown formula, and doubling the number of trials, however, we are able to raise these coefficients to 0.900 and 0.867 respectively; and, since this doubling procedure corresponds to what would be a test-retest situation, it is reasonable to conclude that the instrument satisfies the conventional requirements of reliability.

The average scores for the 45 *Ss* were 9.215 for the right hand and 14.102 for the left hand; the corresponding standard deviations were 3.726 and 5.658, respectively. The difference between these averages is 4.887, and the critical ratio of this difference (quotient of difference by the standard error of the difference) is 4.79, which is high enough to dissipate any doubt about the capacity of the instrument to discriminate handedness.

Decile norms for the average number of contacts in six trials with each hand have been computed and the scores are given in Table I. They are applicable, of course, only to young men and must be taken as approximations until confirmed or corrected by further standardization on larger samplings.

AN INEXPENSIVE DEVICE FOR MEASURING ROTATIONAL SPEED

By CLIFFORD P. SEITZ, Columbia University, and H. B. SHAPIRO, New York City

In searching for equipment with which to measure the rotational speed of motors and disks, the psychologist is compelled to choose from instruments designed for commercial purposes or to construct devices of his own.

At present there are available commercially tachometers which consist of a magneto generator that is directly connected to an indicating instrument calibrated in convenient units such as miles per hour or revolutions per minute. This equipment is expensive and requires reduction gearing for use at low speeds at full scale. There are other possibilities, such as bridge speedometers, which are efficient at high speeds; and pentode tube-meters operating on a condenser discharge principle, with a range of 0–15,000 r.p.s. Neither of the latter is easily constructed outside of a physics laboratory since considerable apparatus is required for calibration. Finally there is the stroboscope which, in its commercial form is expensive and of limited accuracy.

We have found the automobile speedometer a useful instrument for measuring speed in the study of flicker phenomena. Modern speedometers (since 1930) operate on the magnetic principle. A revolving magnet, positively driven from the motor shaft, sets up eddy currents which influence a metal part separated from it by a small air gap. This part is directly connected to the indicating lever which is pivoted in jeweled bearings to increase its sensitivity to small changes in current. An important feature of the design is the linear relation between speed of rotation of the permanent magnet and movement of the indicator. Adjustment is made for changes in drag due to temperature variations. The accuracy of these speedometers is given as $\pm$ 1 % at full scale. This accuracy is as good as is found in the best of the tachometers referred to above and considerably better than some.

These instruments are available new at about $15 from a number of manufacturers. Recent (airplane) models have face dials ranging in size from 2¾ to 4¼ in. in diam. The larger dials are preferable. They may also be bought from a second hand dealer at about one-fifth of this figure. When purchased new they can be obtained with a calibration in revolutions per minute. Usually the scale runs from 0–3500 r.p.m., but other ranges may be obtained. When purchased from a second hand dealer they are, of course, calibrated in miles per hour.

At present we are using a speedometer salvaged from a late model Ford. In making the calibration, the speedometer was connected by a flexible shaft (supplied by the manufacturer in lengths up to 20 ft.) to the motor or rotating shaft. This was managed by imbedding the distal end of the flexible shaft in a small piece of round brass stock and connecting motor shaft and flexible shaft with a section of rubber tubing. A gear (16 teeth, P.D. ½ in.) was fastened to the rotating shaft and meshed with a bronze gear (160 teeth, P.D. 5 in.). This served to reduce the speed of the larger gear ten times. A black paper disc was fastened to the larger,

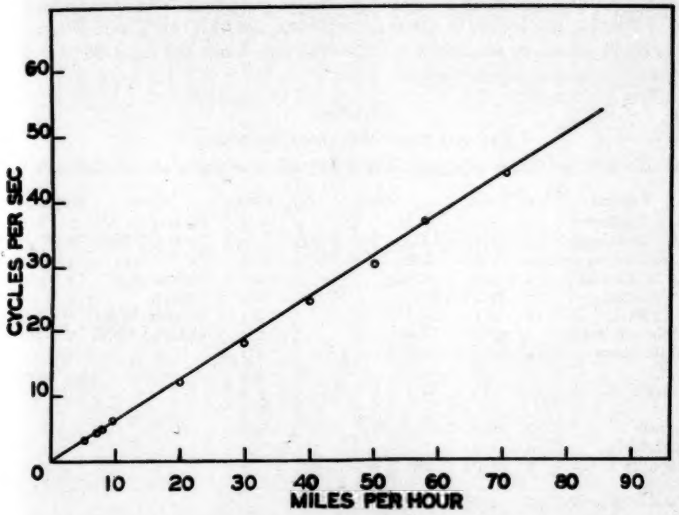

FIG. 1. CALIBRATION OF DEVICE FOR MEASURING ROTATIONAL SPEED

slower gear and marked with a white spot. The time for ten complete cycles at various speeds was obtained by means of a 1/100-sec. chronoscope. The plot of this data is shown in the accompanying figure (Fig. 1). It is to be noted that the values plotted are two times full scale since our rotating disk had two sectors and thus gave twice as many cycles as revolutions per second. The significant thing is the linear relationship between rotation and dial readings.

Such an apparatus has several pronounced advantages. It is inexpensive; rotational speed is read directly; its calibration does not change since the instrument is meant for constant use and is, therefore, ruggedly designed; it may be simply mounted; and it may be illuminated indirectly, an important factor under conditions where it is necessary to keep the subjects dark-adapted.

NOTES AND DISCUSSIONS

Distribution of the Psychologists Starred in the Six Editions of *American Men of Science*

One hundred and eighteen psychologists, selected by a secret vote of the leading research workers in psychology, have been starred in the six editions of Cattell's *American Men of Science* (see List 1). The first group, 50 in number, were selected in 1903 by a dozen recognized leaders in psychology. Subsequently, groups of 12 (1909), 18 (1921), 12 (1927), 13 (1932), and 13 (1937) were selected by vote of all previously starred psychologists and men nominated for starring by one or more of the psychologists already starred.[1]

TABLE I

Regional Yield of Starred Psychologists

(Number born per million of population or immigrants in 1870, the average date of birth.)

Regions	No.	Yield	States	No.	Yield	Cities	No.	Yield
New England	23	6.2	N. H.	3	9.4	Burlington, Vt.	2	100+
Middle Atlantic	22	2.5	Mass.	12	7.9	Boston & Sub.	6	20.0
E. N. Central	29	3.2	Conn.	3	5.6	N. Y. City	4	2.6
W. N. Central	7	1.9	N. Y.	9	2.0	Philadelphia	5	7.0
S. Atlantic	7	1.2	Pa.	10	2.9	Chicago	2	7.0
S. Central	1	0.1	N. J.	3	3.3	Laramie, Wyo.	2	200+
Mountain States	3	10.0	Ohio	7	2.5	Oakland, Calif.	2	100+
Pacific States	3	4.3	Ind.	7	4.1			
			Ill.	10	4.0			
Total U. S.	95	2.5	Wis.	3	3.0			
			Ia.	3	2.5			
Canada	4	8.0	Calif.	3	5.0			
England	4	5.0						
Germany	5	3.0						
Total Immigrants	23	4.0						

List 1. Psychologists Starred 1903-1937

(Name; year of birth and death, if dead; and the edition in which first starred.)

The star indicates that, in the private opinion of his peers, the starred psychologist is distinguished for psychological research. It implies either a large volume of good work or a considerable amount of especially original work. Of course it does not imply that the work done by others is not decidedly worth-while, but merely that it has not impressed the voters as quite so worthy of approbation.

The star is a recognition which not only gives the recipient satisfaction, but also increases his opportunities. It is a challenge to the recipient to continue his good work and to others who aspire to win this recognition. Vast amounts of good work have been completed as a result of friendly rivalry. Many psychologists who are not starred feel confident that they are "as good a man as . . ." and consequently set out to prove it.

The good that starring does is increased by the widened knowledge as to whom are starred and why. This widened knowledge not only encourages and puts the

[1] Cattell's inauguration of the system of starring the leading research workers in each of twelve fundamental sciences is considered by competent judges to have been a major contribution to the growth of research in America.

List 1. Psychologists Starred 1903-1937

(Name; year of birth and death, if dead; and the edition in which first starred.)

Allport, Floyd H., 1890– — , *4
Allport, Gordon W., 1897– — , *6
Anderson, John E., 1893– — , *5
Angell, Frank, 1857– — , *1
Angell, James R., 1869– — , *1
Armstrong, Andrew C., 1860–1935, *1
Baldwin, Bird T., 1875–1928, *3
Baldwin, J. Mark, 1861–1934, *1
Bentley, Madison, 1870– — , *2
Bingham, Walter V., 1880– — , *3
Bolton, Thaddeus L., 1865– — , *1
Boring, Edwin G., 1886– — , *3
Brown, Warner, 1882– — , *4
Bryan, William L., 1860– — , *1
Buchner, Edward F., 1868–1929, *1
Calkins, Mary W., 1863–1930, *1
Carmichael, Leonard, 1898– — , *6
Carr, Harvey, 1873– — , *3
Cattell, J. McKeen, 1860– — , *1
Creighton, James E., 1861–1924, *1
Dallenbach, Karl M., 1887– — , *5
Dashiell, John F., 1888– — , *5
Dearborn, Walter F., 1878– — , *3
Delabarre, Edmund B., 1863– — , *1
Dewey, John, 1859– — , *1
Dodge, Raymond, 1871– — , *1
Downey, June E., 1875–1932, *4
Dunlap, Knight, 1875– — , *3
Farrand, Livingston, 1867– — , *1
Fernberger, Samuel W., 1887– — , *5
Ferree, Clarence E., 1877– — , *3
Freeman, Frank N., 1880– — , *5
Fullerton, George S., 1859–1925, *1
Gardiner, Harry N., 1855–1927, *1
Garrett, Henry E., 1894– — , *6
Gates, Arthur I., 1890– — , *4
Gesell, Arnold, 1880– — , *4
Goddard, Henry H., 1866– — , *3
Goodenough, Florence L., 1886– — , *6
Hall, G. Stanley, 1846–1924, *1
Harris, William T., 1835–1909, *1
Hollingworth, Harry L., 1880– — , *3
Holt, Edwin B., 1873– — , *2
Howes, Ethel P., 1872– — , *2
Hull, Clark L., 1884– — , *5
Hunter, Walter S., 1889– — , *3
Hurd, Henry M., 1843–1927, *2
Hyslop, James H., 1854–1920, *1
James, William, 1842–1910, *1
Jastrow, Joseph, 1863– — , *1
Johnson, Harry M., 1885– — , *5
Jones, Harold E., 1894– — , *6
Judd, Charles H., 1873– — , *1
Kelley, Truman L., 1884– — , *4
Klüver, Heinrich, 1897– — , *6
Koffka, Kurt, 1886– — , *5
Köhler, Wolfgang, 1887– — , *6
Ladd-Franklin, Christine, 1847–1930, *1
Ladd, George T., 1842–1921, *1
Landis, Carney, 1897– — , *6
Langfeld, Herbert S., 1879– — , *3
Lashley, Karl S., 1890– — , *4
Leuba, James H., 1868– — , *1
Lewin, Kurt, 1890– — , *6
Lindley, Ernest H., 1869– — , *1
MacDougall, Robert, 1866– — , *1
McDougall, William, 1871–1938, *4
McGeoch, John A., 1897– — , *6
Marshall, Henry R. 1852–1927, *1
Martin, Lillien J., 1851– — , *2
May, Mark A., 1891– — , *5
Meyer, Adolf, 1866– — , *1
Meyer, Max F., 1873– — , *1
Miles, Walter R., 1885– — , *4
Münsterberg, Hugo, 1863–1916, *1
Newbold, William R., 1865–1926, *1
Nichols, Herbert, 1852–1936, *1
Ogden, Robert M., 1877– — , *3
Pace, Edward A 1861– — , *2
Paterson, Donald G., 1892– — , *5
Patrick, George T., 1857– — , *1
Peterson, Joseph, 1878–1935, *4
Pierce, Arthur H., 1867–1914, *1
Pillsbury, Walter B., 1872– — , *1
Pintner, Rudolph, 1884– — , *3
Poffenberger, Albert T., 1885– — , *4
Robinson, Edward S., 1893–1937, *5
Sanford, Edmund C., 1859–1924, *1
Santayana, George, 1863– — , *1
Schurman, Jacob G., 1854– — , *1
Scott, Walter D., 1869– — , *3
Scripture, Edward W., 1864– — , *1
Seashore, Carl E., 1866– — , *1
Sidis, Boris, 1867–1923, *2
Starbuck, Edwin D., 1866– — , *2
Starch, Daniel, 1883– — , *3
Stone, Calvin P., 1892– — , *5
Stratton, George M., 1865– — , *1
Strong, Charles A., 1862– — , *1
Strong, Edward K., 1884– — , *3
Terman, Lewis M., 1877– — , *3
Thorndike, Edward L., 1874– — , *1
Thurstone, Louis L., 1887– — , *4
Titchener, Edward B., 1867–1927, *1
Tolman, Edward C., 1886– — , *5
Warren, Howard C., 1867–1934, *1
Washburn, Margaret F., 1871– — , *1
Watson, John B., 1878– — , *2
Wells, Frederic L., 1884– — , *2
Wertheimer, Max, 1880– — , *6
Wever, Ernest H., 1902– — , *6
Whipple, Guy M., 1876– — , *2
Witmer, Lightner, 1867– — , *1
Wolfe, Harry K., 1858–1918, *1
Woodrow, Herbert, 1883– — , *6
Woodworth, Robert S., 1869– — , *1
Woolley, Helen T., 1874– — , *3
Yerkes, Robert M., 1876– — *2

starred men more fully on their mettle, but it also attracts attention to their work, and increases their opportunities for further research. It, moreover, augments the opportunities of promising persons not starred in the hope that, as a consequence of encouragement and improved facilities, they will win this coveted recognition. The various universities employing starred scientists are placing increased value upon this recognition as a proof of individual merit and institutional strength. They not only attempt to retain and attract men already starred, but also to have local men not yet starred win this high honor; to this end they often increase facilities and otherwise encourage their more promising men.

Birth. (a) Place. Table I and List 2 disclose that 95 of the 118 starred psychologists were native-born. Of these 95, the East North Central States yielded 29, New England 23, the Middle Atlantic States 22, the West North Central and the South Atlantic each 7, the Mountain and Pacific States each 3, and the South Central States 1. In proportion to population in 1870, the average date of birth, New England yielded more than twice as many as did the North Central and Middle Atlantic States, and ten times as many as the South. The Mountain and Pacific States had only small populations in 1870, and their combined yield was almost at New England's rate.

The leading states, in number produced, are Massachusetts 12, Illinois and Pennsylvania 10, New York 9, Indiana and Ohio 7, and six states with 3 each (California, Connecticut, Iowa, New Hampshire, New Jersey, and Wisconsin). Five other states yielded 2 each (Maine, Michigan, South Carolina, Vermont, and Wyoming). Twelve states yielded 1 each and the remaining nineteen states, none. Most of these latter are in the South Central section or in the West. The West had few people when most of the starred psychologists were born.

LIST 2. BIRTHPLACES: SECTION, STATE, AND CITY

New England, total 23

Maine (2)	*Dover:* Delabarre; *Vassalboro:* Goddard.
New Hampshire (3)	*Mason:* Scripture; *North Boscawen:* Patrick; *Walpole:* Nichols.
Vermont (2)	*Burlington:* Angell, J. R., Dewey.
Massachusetts (12)	*Ashfield:* Hall; *Belchertown:* Woodworth; *Boston:* Wells; *Danvers:* Whipple; *Farmingham:* Howes; *Haverhill:* Strong, C. A.; *Marblehead:* Dearborn; *Williamsburg:* Thorndike; *Westboro:* Pierce; *West Newton:* Tolman; *Winchester:* Holt; *Woburn:* Dodge.
Connecticut (3)	*Hartford:* Calkins; *North Killingsley:* Harris; *Windsor:* Ladd-Franklin.
Rhode Island (1)	*South Scituate:* Angell, F.

Middle Atlantic States, total 22

New York (9)	*Akron:* Hull; *Argyle:* McGeoch; *Binghampton:* Ogden; *New York City:* Armstrong, James, Marshall, Washburn; *Olean:* Martin; *Syracuse:* Strong, E. K.

Pennsylvania (10)	*Breadysville:* Yerkes; *Dauphin:* Poffenberger; *Easton:* Cattell; *Honesdale:* Goodenough; *Marshalton:* Baldwin, B. T.; *Philadelphia:* Boring, Carmichael, Fernberger, Langfeld, Witmer.
New Jersey (3)	*Montclair:* Warren; *Newark:* Farrand; *New Brunswick:* Jones.

East North Central States, total 29

Ohio (7)	*Chillicothe:* Woodrow; *Columbus:* Paterson; *Lebanon:* Robinson; *Painesville:* Ladd; *Sidney:* Ferree; *West Alexandria:* Landis; *Xenia:* Hyslop.
Indiana (7)	*Bloomington:* Bryan; *Bridgeport:* Starbuck; *Johnson Co.:* Terman; *Montezuma:* Allport, G. W.; *Paoli:* Lindley; *Portland:* Stone; *Southport:* Dashiell.
Illinois (10)	*Benton:* Wever; *Bloomington:* Wolfe; *Champaign:* Dallenbach; *Chicago:* Thurstone, Wooley; *Cooksville:* Scott; *Decatur:* Hunter; *Morris:* Carr; *Paxton:* Buchner; *Sonora:* Bolton.
Michigan (2)	*Muskegon:* Kelley; *Union City:* Hurd.
Wisconsin (3)	*Alma:* Gesell; *La Crosse:* Starch; *Milwaukee:* Allport, F. H.

West North Central States, total 7

Minnesota (1)	*Red Wing:* Gates.
Iowa (3)	*Burlington:* Pillsbury; *Clinton:* Bentley; *Swan Lake:* Bingham.
Missouri (1)	*Nelson:* Johnson.
North Dakota (1)	*Silverleaf:* Miles.
Nebraska (1)	*De Witt:* Hollingworth.

South Atlantic States, total 7

Delaware (1)	*Wilmington:* Newbold.
Virginia (1)	*Clover:* Garrett.
West Virginia (1)	*Davis:* Lashley.
South Carolina (2)	*Columbia:* Baldwin; *Greenville:* Watson.
Georgia (1)	*Greensboro:* Brown.
Florida (1)	*Starke:* Pace.

East South Central States, total 1

Tennessee (1)	*Jonesboro:* May.

Mountain States, total 3

Wyoming (2)	*Laramie:* Anderson, Downey.
Utah (1)	*Huntsville:* Peterson.

Pacific States, total 3

California (3)	*Diamond Springs:* Dunlap; *Oakland:* Sanford, Stratton.

Foreign-born, total 23	
India (2)	Fullerton, Judd.
Canada (4)	*Prince Edwards Island* (Freetown): Schurman; *Nova Scotia* (Pictou): Creighton; *Ontario* (Rockwood): Freeman; *Quebec* (Dewittville): MacDougall.
England (4)	*Norwich:* Gardiner; *Lancashire:* McDougall; *Lytham:* Pintner; *Chichester:* Titchener.
Germany (5)	Klüver; Lewin; Münsterberg; (*Berlin*): Koffka; (*Danzig*): Meyer, M.
Russia (3)	*Estonia* (Reval): Köhler; *Poland:* Sidis; (*Warsaw*): Jastrow.
Switzerland (2)	*Neuchatel:* Leuba; *Niederweningen:* Meyer, A.
Czechoslovakia (1)	*Prague:* Wertheimer.
Sweden (1)	*Morlunda:* Seashore.
Spain (1)	*Madrid:* Santayana.

In proportion to their population in 1870, Wyoming, New Hampshire, Massachusetts, Connecticut, and California each yielded more than twice the national average. Next in order were Indiana, Illinois, New Jersey, and Wisconsin, with yields of 1.6 to 1.2 times the national average. Ohio and Iowa yielded at the national average rate while Pennsylvania yielded at somewhat above, and New York somewhat below that rate.

Of the cities and towns which yielded two or more, Laramie, Wyoming, did best in proportion to population, followed by Burlington, Vermont, and Oakland, California. Of the larger cities, Boston did nearly three times as well as Philadelphia or Chicago and seven times as well as New York.

The 23 foreign-born starred psychologists came from several countries, 4 each from Canada and England, 5 from Germany, 3 from pre-war Russia, 2 from Switzerland, 2 from British India (children of American missionaries) and 1 each from Sweden, Spain, and Prague. In proportion to the number of immigrants in America in 1880, immigrants yielded future starred psychologists at a notably higher rate than the national average yield, Canada at three times that rate, England at twice, Germany at one-fifth more.

(b) Trends in yield. The foregoing analysis of place of birth is based on the totals without regard to time of birth. As the older starred psychologists were born more than a half century before the younger ones, an analysis of yields by decade of birth has been made. A table, not published here, reveals several significant trends.

Ten of the 23 foreign-born were born in the 1860's, 5 in the 1880's, 3 each in the 1850's and 1870's, and 2 in the 1890's. In other words, nearly a third of the older group were foreign-born, a fifth of the intermediate group, but only an eighth of the younger group.

Half of the group born in the 1860's were born in cities; about three-fourths of those born in the 1870's and the 1880's, and nearly seven-eighths of the youngest group were urban born. Large cities yielded about a seventh of both the older and intermediate groups, but only one of the youngest twenty. New York City has yielded none since 1871, Boston and Chicago none since 1887. Philadelphia has yielded one or two in each decade since the 1850's.

New England led in the number born during each decade until the 1880's when it was surpassed by the Middle Atlantic, the East North Central and West North Central regions each of which yielded 6 in contrast with New England's 2. For the 1890's, the East North Central region with 6 led in numbers, the Middle Atlantic yielded 3, the West North Central region and South Atlantic each 2 and the Mountain and South Central 1, the Pacific and New England States none. The Pacific States' contributions were born one each in the 1850's, 1860's, and 1870's. The only starred psychologist born after 1899 was born in 1902 in Illinois.

A national decline in the yield of subsequently starred psychologists in proportion to population between the 1860's and the 1890's is due to the fact that the number of starred men has been held constant while the population more than doubled. This means that stars are increasingly difficult to win. At the first starring in 1903 when 50 were starred, about one-fourth of all the fairly well-qualified men were starred, but at the 1937 starring, when only 13 were starred, less than one-tenth of the younger non-starred psychologists who had accomplished considerable amounts of good work were starred.

(c) Possible explanations of the trends. The regional variations in yield from decade to decade, disclosed by the analysis of birth by decades, are partly due to the statistical inadequacy of the data. For example, when starred psychologists were produced at the rate of only one per five million people, as in the 1890's, the fact that none was born in the Pacific States, which had in 1895 only about two million people, may not be statistically significant. Similarly as New England had only about five million people in 1895, the fact that no starred psychologist was born in this region in the 1890's may not indicate sterility: one or more may win a star at a future date. It is evident, however, that there has been a real decline in New England in proportion to population since the 1870's and in the Midwest since the 1880's.

Several explanations may be offered of such progressive changes in yield in proportion to population as have occurred with respect to native-born starred psychologists. Three suggestions are: (1) The increase in yield in proportion to population evident for the East North Central States to the 1860's and for the West North Central States to the 1870's may be an indication that in order to produce more than a few leaders, a certain amount of sociological, educational maturity in local 'culture' is necessary. If so, this would help to explain the fact that none of the starred psychologists was born in the more sparsely settled northern *halves of the populations* of Michigan, Wisconsin or Minnesota, or in mining camps in the West. (2) An analogous theory is that the subsequent decline is partly due to the 'softening' which often occurs in a locality after a certain amount of culture and wealth has been attained. (3) A third theory is that the change in yield reflects an actual change in the intellectual ability of the people of the area. Rapid improvement in average intellectual ability is chiefly due to the migration hence of many superior people. Correspondingly, a subsequent regional decline in ability is due partly to migration of the superior people from the area, and partly to the greater decline in the birthrate of the superior than of the inferior levels of the population. Certain it is that hundreds of thousands of exceptionally alert Yankees moved from New England to the Midwest in the 1840's, 1850's, and 1860's. Cattell from a special questionary found, and much other evidence indicates, that a large majority of the leaders of the Midwest were of at least half Yankee descent. Likewise, a larger

share of the immigrants from the 1840's-1860's were superior intellectually than has been true for later decades when immigration was easier, and manual laborers vastly predominated. Moreover, most of the earlier immigration came from northwestern Europe and Canada but an increasing majority of the later immigrants came from other parts of Europe, from which regions relatively few American leaders have come. The great growth of cities, with their large number of manual laborers in proportion to population, have likewise often depressed the intellectual level.

Another factor which perhaps helps to explain the variation in yield from time to time, as well as from place to place, is in the local demand. There is a rough correlation between use and yield (see Table VI). Local men of an age to fill new positions may be given the opportunity, and may ultimately win the national recognition.

Cattell has expressed the opinion that the most important factor is opportunity; that when good opportunities are available, men qualified to use them arise. This optimistic theory is not very well supported by the present body of data. For example, New England, New York City, and California employ a large share of the younger starred psychologists (born after 1889), but have yielded none.

The influence of leadership helps greatly to explain the distribution of place of training of subsequent leaders, and to some extent, place of birth, as relatively few have gone far from their birthplaces for their early training. For example, Wyoming's high record in proportion to population is partly due to the late Miss Downey's presence and influence at the University of Wyoming. Likewise Indiana's better yield as compared with Michigan (7 vs. 2) is partly attributable to David Starr Jordan, highly stimulating teacher of 3 and former employer (as president of Indiana University and Stanford) of 4 of Indiana's 7.

Hence, the explanation of the unequal distribution of the yield of these notables is not simple nor clear, despite the great social desirability of an understanding of what conditions are conducive to the yield of leaders. Nevertheless, such data as these help to illuminate the problem and bring its solution nearer.

Collegiate training. (a) Undergraduate education. For their undergraduate education, the 102 starred psychologists who graduated from American colleges are alumni of 54 colleges. Thirteen institutions graduated 3 or more, a total of 54: 5 each from Amherst, California, Harvard, Indiana, and Pennsylvania; 4 each from Chicago, Michigan, Nebraska, Princeton, and Wesleyan; 3 each from Columbia, Cornell, and Vassar. Seven institutions graduatcd 2 each (Haverford, Illinois, Ohio, Smith, Vermont, Williams, and Wyoming) and 34 graduated one. (See List 3.)

As to type of American college from which the starred psychologists graduated (bachelor's degree), 43 were from small colleges, 52 from endowed universities, of whom 35 were from state universities. The relative importance of the small college was greatest as to the older men—three times as many born before 1860 graduated from small colleges as from universities. For those born in the 1860's, 1870's, 1890's, colleges and universities about tied in the number graduated; for those born in the 1880's, the universities trained twice as many as did the small colleges—chiefly because of the large number graduated from state universities. State universities graduated a half of those born in the 1850's, about a third of those born in the 1860's, 1870's and 1890's, but nearly a half of those born in the 1880's.

Foreign universities gave collegiate training to one-fifth of those born in the 1860's and 1880's, but to only about one out of twelve of those born in the 1870's or since 1887.

Even those starred psychologists who were born in the 1840's were college graduates and also had some graduate work.

Considerably more than a half of the subsequently starred psychologists had their college work in an institution which had at the time a distinguished psychologist professor.

An analysis by decades reveals that Princeton's four alumni were born in the 1860's, Harvard's five include one born in the 1840's, two in the 1860's, one in the 1870's and two in the 1890's; Columbia's three include one in the 1850's, two in the 1880's; Indiana's five include three born in the 1860's, one in the 1870's and one in the 1890's.

(b) Graduate work. Table II shows how many starred psychologists had graduate work at each of the more important universities. The leaders are: Harvard 21, Columbia 19, Chicago 12, Clark 10, Hopkins 9, Cornell 8 (33 had some graduate work in Germany).

(c) Doctoral training. List 4 and Tables II and III present data regarding the doctorates of starred psychologists. They reveal that Columbia and Harvard have conferred the doctorate on 15, Chicago on 11, Clark on 9, and Cornell and Hopkins on 8. German universities conferred the doctorate on 20.

When classified as to time, it is seen that for the older starred psychologists, German universities, Hopkins, Clark and Harvard were predominant; for those born in the 1870's and 1880's, Chicago and Cornell were followed by Columbia and Harvard; for those born since 1889, Harvard has led, followed by Columbia.

A third of the 12 born in the 1850's and two of the 33 born in the 1860's obtained no doctorate, and only one born since 1871 has been starred without a doctorate.

List 3. Institutions Conferring Baccalaureate Degree

Conferred upon three or more:

Amherst: Delabarre, Gardiner, Jones, Pierce, Woodworth; *California:* Brown, Dunlap, Sanford, Stratton, Strong, E. K.; *Chicago:* May, Peterson, Wooley, Yerkes; *Columbia:* Goodenough, Marshall, Wells; *Cornell:* Boring, Ogden, Thurstone; *Harvard:* Allport, F. N., Allport, G. W., Holt, Santayana, Sidis; *Indiana:* Bryan, Lindley, Starbuck, Stone, Terman; *Michigan:* Angell, J., Bolton, Hull, Hurd, Woodrow; *Nebraska:* Bentley, Hollingworth, Pillsbury, Wolfe; *Pennsylvania:* Fernberger, Fullerton, Jastrow, Newbold, Witmer; *Princeton:* Armstrong, Baldwin, M., Farrand, Warren; *Vassar:* Ladd-Franklin, Martin, Washburn; *Wesleyan:* Dearborn, Freeman, Judd, Thorndike.

Conferred upon two:

Haverford: Goddard, Langfeld; *Illinois:* Dallenbach, Kelley; *Ohio State:* Landis, Paterson; *Smith:* Calkins, Howes; *Vermont:* Angell, F., Dewey; *Williams:* Dodge, Hall; *Wyoming:* Anderson, Downey.

Conferred upon one:

Beloit: Bingham; *Brown:* Whipple; *Bucknell:* Poffenberger; *Cincinnati:* Robinson; *Colorado:* Carr; *Dalhousie:* Creighton; *Earlham:* Miles; *Evansville:* Dashiell; *Gustavus Adolphus:* Seashore; *Illinois College:* Wever; *Iowa:* Patrick; *Lafayette:* Cattell; *Leander Clark:* Buchner; *Maryville:* May; *Massachusetts Technology:* Tolman; *Missouri Valley:* Johnson; *Morningside:* Starch; *New York College:* Scripture; *Northwestern:* Scott; *Ohio Wesleyan:* Ferree; *Pacific:* Miles; *Richmond:* Garrett; *Rochester:* Strong; *St. Charles:* Pace; *Swarthmore:* Baldwin, B.; *Texas:* Hunter; *Tufts:* Carmichael; *Valparaiso:* Stone; *Westminster:* McGeoch; *Western Reserve:* Ladd; *West Virginia:* Lashley; *Wisconsin:* Gesell; *Wooster:* Hyslop; *Worcester:* Nichols.

Conferred in Europe:

Klüver, Koffka, Köhler, Leuba, Lewin, MacDougall, McDougall, Meyer, A., Meyer, M., Münsterberg, Pintner, Schurman, Titchener, Wertheimer.

LIST 4. UNIVERSITIES CONFERRING THE DOCTORATE

(Numerals indicate edition in which first starred.)

Conferred on eight or more:

Chicago: Bingham 3, Carr 3, Downey 4, Hunter 3, McGeoch 6, Peterson 4, Robinson 5, Thurstone 4, Watson 2, Wooley 3, Yerkes 2. *Clark:* Bolton 1, Bryan 1, Gesell 4, Goddard 3, Leuba 1, Lindley 1, Nichols 1, Starbuck 2, Terman 3. *Columbia:* Brown 4, Dashiell 5, Dearborn 3, Garrett 6, Gates 4, Hollingworth 3, Jones 6, Kelley 4, May 5, Poffenberger 4, Strong, E. 3, Thorndike 1, Wells 2, Woodrow 6, Woodworth 1. *Cornell:* Bentley 2, Boring 3, Creighton 1, Dallenbach 5, Ferree 3, Pillsbury 1, Washburn 1, Whipple 2. *Harvard:* Allport, F. 4, Allport, G. 6, Anderson 5, Baldwin 3, Carmichael 6, Dunlap 3, Hall 1, Holt 2, James 1, MacDougall 1, Pierce 1, Santayana 1, Sidis 2, Tolman 5, Wever 6. *Hopkins:* Dewey 1, Hyslop 1, Jastrow 1, Johnson 5, Lashley 4, Patrick 1, Sanford 1, Warren 1.

Conferred on three or less:

Iowa: Miles 4, Starch 3. *Minnesota:* Landis 6, Stone 5. *Pennsylvania:* Fernberger 5, Newbold 1. *Stanford:* Goodenough 6, Klüver 6. *Yale:* Buchner 1, Freeman 5, Seashore 1.

Conferred in Europe:

Angell, F. 1, Cattell 1, Delabarre 1, Dodge 1, Judd 1, Koffka 5, Köhler 6, Langfeld 3, Lewin 6, Meyer, M. 1, Münsterberg 1, Ogden 3, Pace 2, Pintner 3, Schurman 1, Scott 3, Scripture 1, Stratton 1, Titchener 1, Wertheimer 6, Witmer 1, Wolfe 1.

Endowed universities conferred the doctorate upon 17 times as many of those born in the 1860's or 1870's as did the state universities, upon 5 times as many of those born in the 1880's, upon 7 times as many for those born since 1889. Half of Hopkins' eight starred doctors were born in the 1850's and a fourth in the 1860's. Six of Clark's 9 were born in the 1860's and one in the 1850's. (Four of the 9 were

Hoosiers and graduates from Indiana University.) Columbia's starred doctors are mostly fairly young; half were born in the 1880's and one-fourth in the 1890's. Harvard has had two periods of relatively large output: first, men born in the 1860's and early 1870's who came under the influence of William James; and secondly, men born since 1889—notable is the fact that Harvard conferred the doctorate upon 7 of the 17 born since 1889 who have won stars. Columbia with 4 stands next. Cornell conferred doctorates upon 5 born in the 1870's, 1 in the 1860's, 2 in the 1880's; Chicago on 5 born in the 1870's, 3 in the 1880's, and 1 in the 1890's.

TABLE II

INSTITUTIONS AT WHICH STARRED PSYCHOLOGISTS WERE TRAINED AND AT WHICH THEY ARE OR WERE EMPLOYED

Institution	Trained			Employed		
	Bacca.	Grad. stud.	Ph.D.	1938	retired	dead
California	5	3	0	4	1	0
Chicago	4	12	11	4	1	0
Clark	0	10	9	0	0	2
Columbia	3	19	15	7	3	2
Cornell	3	8	8	3	3	1
Harvard	5	21	15	5	1	3
Hopkins	0	9	8	2	0	3
Illinois	2	2	0	1	0	0
Indiana	5	4	0	0	1	0
Iowa	1	3	2	2	1	1
Michigan	5	2	0	1	0	0
Minnesota	0	2	2	3	0	0
Nebraska	4	0	0	0	0	1
Pennsylvania	5	5	2	1	1	1
Princeton	4	3	1	2	1	1
Ohio	2	1	0	1	0	0
Stanford	0	2	2	3	2	0
Smith	2	1	0	1	1	2
Vassar	3	1	0	0	1	0
Wesleyan	4	1	0	1	0	1
Wisconsin	1	1	1	0	1	0
Wyoming	2	1	0	0	0	1
Yale	0	4	3	5	2	1
Europe	14	33	22			

College graduates only: Amherst 5, Haverford 2, Vermont 2.

There has been a progressive decline in the percentages of the doctorates received in Germany. For those born in the 1850's, German universities conferred nearly one-half of the doctorates; for those born in the 1860's, just a third, for those of the 1870's, just a fourth, for those of the 1880's a sixth, and for those born since 1887, only one out of the 19, and he was born and trained in Germany.

For their doctorates, most subsequently starred psychologists went to a university with an outstanding leader. G. Stanley Hall conferred the doctorate upon the largest number of subsequently distinguished psychologists—6 at Hopkins and 9 at Clark. The fact that during his last 13 years at Clark (1907-1920), while he was 61-74 yr. old, none were graduated presumably illustrates the serious handicap of increasing age with the consequently increasing gap between professor and student.

During a dozen years while he was young, Hall had graduated nearly a dozen men who subsequently became distinguished psychologists.

Employment. List 5 presents the place of employment of most of the starred psychologists employed in 1938, together with the connections of those retired, and for those dead, and place of employment in their later years. This list is summarized in Table IV, which makes conspicuous the leading universities in this respect; namely, Columbia, Harvard, California, Chicago, and Yale. The rank of these leaders is not changed if only the men starred in 1921-1937 are counted, nor are important changes called for if the retired men are also counted. Of the psychologists starred in 1932 or 1937, Minnesota, however, has the most (3), followed by California and Yale, with 2 each.

TABLE III

UNIVERSITY AT WHICH DOCTORATE WAS OBTAINED AND DATE FIRST STARRED

University	Date starred						1921–1937
	1903	1909	1921	1927	1932	1937	
Chicago	0	2	4	3	1	1	9
Clark	5	1	2	1	0	0	3
Columbia	2	1	3	4	2	3	12
Cornell	3	2	2	0	1	0	3
Harvard	5	2	2	1	2	3	8
Hopkins	6	0	0	1	1	0	2
Iowa	0	0	1	1	0	0	2
Minnesota	0	0	0	0	1	1	2
Pennsylvania	1	0	0	0	1	0	1
Yale	2	0	0	0	1	0	1
Europe	13	1	4	0	1	3	8

As to trends in employment, endowed universities have employed more than a half of each age-group; of those born before 1860 or after 1889 the ratio is almost three-fourths. State universities employed nearly one-fourth of each age-group since the 1850's, except during the 1870's when they employed one-sixth. Endowed colleges and commercial work have employed almost the same numbers, from one to three of each decade-age-group, except that none of the youngest group is in commercial work.

Age at starring. List 1 names the psychologists starred in every edition. Table V classifies by ages at starring the psychologists honored at each of the starrings and also by decade of birth. It reveals that the median age at starring has risen from about 40 yr. in 1903 to about 44 yr. for the last three starrings. Since 1921, only one has been starred while under 37 yr.; he was 35.

The lower part of Table V shows the number born in each of the four chief decades of birth and their age at starring. Those born in the 1860's or 1870's were starred at a median age of 39 yr. while those born in the 1880's and 1890's were starred at median ages of 44 and 40 yr. Only two have been starred while under 31 yr.; one of these was Thorndike, the other was Wells.

The eight women who have won stars in psychology were starred at 30, 37, 40, 47, 51, 52, 56, and 58 yr. Thus their median age was several years above that for the men. Three were starred in 1903, 2 in 1909, 1 each in 1921, 1927 and 1937.

TABLE IV

PLACE OF EMPLOYMENT IN 1938 AND DATE FIRST STARRED

University	Date starred						1921–1937	Retired
	1903	1909	1921	1927	1932	1937		
California	0	0	1	1	1	1	4	1
Chicago	0	0	2	1	0	1	4	1
Columbia	2	0	2	2	0	1½	5½	3
Cornell	0	1	1	0	1	0	2	2
Harvard	0	0	2	2	0	1	5	1
Hopkins	1	0	1	0	0	0	1	0
Iowa	1	0	0	0	0	1	1	1
Minnesota	0	0	0	0	2	1	3	0
Princeton	0	0	1	0	0	1	2	1
Stanford	0	0	2	0	1	0	3	2
Yale	0	1	0	2	2	0	4	2

LIST 5. PLACE OF EMPLOYMENT IN 1938 OF STARRED PSYCHOLOGISTS

(Numerals indicate edition in which first starred.)

Institutions employing three or more:

California: Brown 4, Dunlap 3, Jones 6, Tolman 5; retired, Stratton. *Chicago:* Carr 3, Freeman 3, Klüver 6, Thurstone 4; retired, Judd. *Columbia:* Garrett 6, Gates 4, Hollingworth 3, Landis (½) 6, Pintner 3, Poffenberger 4, Thorndike 1, Woodworth 1; retired, Cattell, Dewey, Strong, C.; dead, Ladd-Franklin, Fullerton. *Cornell:* Bentley 2, Dallenbach 5, Ogden 3; retired Farrand, Schurman; dead, Titchener. *Harvard:* Allport, G. 6, Boring 3, Dearborn 3, Kelley 4, Lashley 4; retired, Santayana; dead, James, Münsterberg. *Hopkins:* Ferree 3, Meyer, A. 1; dead, Baldwin, M., Buchner, Hurd. *Iowa:* Lewin 6; retired, Seashore, Patrich; dead, Baldwin, B. *Minnesota:* Anderson 5, Goodenough 6, Paterson 5. *Pennsylvania:* Fernberger 5; retired, Witmer; dead, Newbold. *Princeton:* Langfeld 3, Wever 6; retired, Holt; dead, Warren. *Stanford:* Stone 5, Strong, E. K. 3, Terman 3; retired, Angell, F., Martin. *Smith:* Koffka 5; retired, Howes; dead, Gardner, Pierce. *Yale:* Gesell 4, Hull 5, May 5, Miles 4, Yerkes 2; retired, Angell, J., Dodge; dead, Robinson.

Institutions employing one:

Brown: Hunter 3; *Illinois:* Woodrow 6; *North Carolina:* Dashiell 5; *Northwestern:* Scott 3; *Ohio:* Goddard 3; *Syracuse:* Allport, F. 4; *Swarthmore:* Köhler, 6; *Tufts:* Carmichael 6; *Wesleyan:* McGeoch 6.

In commercial work:

Starch 3, Watson 2, Whipple 2.

In governmental work:

Johnston 5, Landis (½) 6, Wells 2.

Summary. Table VI affords a summary of the contributions of the different regions and states to the list of starred psychologists. It reveals a rough correspondence between the number born, trained, and employed. Those regions and states which

have employed relatively few have, in general, trained relatively few, and have been the birthplaces of few. The three New England states which employ none yielded, however, seven; and several other states have yielded several times as many as they have employed, *e.g.* Indiana and Ohio. California, Connecticut, and Minnesota have conversely employed three times as many as they have yielded.

Table II reveals that several institutions which gave undergraduate training to relatively many who were subsequently starred, conferred the doctorate on none. Examples are California, Indiana, Michigan, Nebraska, and Vassar. Conversely, some universities gave doctoral training to several, but undergraduate training to none. Examples are Clark, Hopkins, and Yale. Sizable discrepancies between the number of undergraduate and doctoral alumni occur also as to Columbia, Harvard, and Chicago.

TABLE V

AGE WHEN STARRED

Date starred	1903	1909	1921	1927	1932	1937
Total number	50	12	18	12	13	13
Median age	40	39	42	44	45	43
% between 35–45 yr.	58	44	57	58	61	61
% under 35 yr.	18	27	11	0	0	0
% over 50 yr.	14	17	11	17	8	23
Number above 55 yr.	5	2	0	1	0	1
Maximum age	68	66	55	56	52	57
Next oldest	61	58	54	52	48	54
Number below 40 yr.	23	6	7	2	2	2
Minimum age	29	25	32	37	39	35
Next youngest	30	31	33	37	39	39

Date born (in decade)	1860's	1870's	1880's	1890's
Total number	33	24	26	16
% between 35–45 yr.	85	34	54	94
% under 35 yr.	9	37	13	0
% over 50 yr.	6	12	16	0
Number above 55 yr.	0	1	1	0
Maximum age	55	56	57	47
Next oldest	52	54	54	43
Number below 40 yr.	17	13	6	6
Minimum age	34	29	25	37
Next youngest	34	30	32	39
Median age	39	39	44	40

(One was born in 1902, starred at 35.)

Slight correspondence between the number given the doctorate and the number employed in 1938 exist for Clark, Hopkins, Harvard, and Chicago. A fairly close ratio prevails as to Iowa, Minnesota, Stanford, and Yale.

Conclusions. The distribution of the places of birth, training and employment of starred psychologists supports several tentative conclusions drawn on the basis of earlier studies of the distribution of notables.[3] Indeed, since the validity of con-

[3] Cf. S. S. Visher, A study of the type of birthplace and of the occupation of fathers of subjects of sketches in *Who's Who in America, Amer. J. Sociol.*, 30, 1925, 551-557; Distribution of women sketched in *Who's Who in America, Scient. Mo.*, 15, 1922, 443-447; Contrasts among Indiana counties in their yield of prominent persons, *Proc. Indiana Acad. Sci.*, 38, 1929, 217-224; Ecology of American notables, *Human Biol.*, 1, 1929, 544-554; The comparative rank of American States, *Amer. J. Sociol.*,

TABLE VI

CONTRIBUTIONS MADE BY THE DIFFERENT REGIONS TO THE LIST OF STARRED PSYCHOLOGISTS
(Number born, trained, and employed.)

Region or State	Born	Trained			Employed	
		Bacca.	Grad. stud.	Ph.D.	1938	ret. or dead
NEW ENGLAND	23	24	37	27	16	15
Maine	2	0	0	0	0	1
New Hampshire	3	0	0	0	0	0
Vermont	2	2	0	0	0	0
Massachusetts	12	17	31	24	9	9
Rhode Island	1	1	0	0	1	1
Connecticut	3	4	5	3	6	4
MIDDLE ATLANTIC	22	26	37	26	20	19
New York	9	11	28	23	15	12
New Jersey	3	4	3	1	3	3
Pennsylvania	10	11	6	2	2	4
EAST NORTH CENTRAL	29	27	21	22	8	4
Ohio	7	5	1	0	1	1
Indiana	7	7	4	0	0	1
Illinois	10	8	13	11	6	1
Michigan	2	5	2	0	1	0
Wisconsin	3	2	1	1	0	1
WEST NORTH CENTRAL	7	8	5	4	6	2
Minnesota	1	1	2	2	3	0
Iowa	3	2	3	2	2	2
Missouri	1	1	0	0	0	0
Nebraska	1	4	0	0	0	1
SOUTH ATLANTIC	7	4	9	8	4	3
Maryland	0	1	9	8	2	1
Dist. of Columbia	0	0	0	0	1	1
Virginia	1	1	0	0	0	0
West Virginia	1	1	0	0	0	0
South Carolina	2	1	0	0	0	0
Florida	1	0	0	0	1	0
SOUTH CENTRAL	1	1	0	0	0	1
Tennessee	1	0	0	0	0	1
MOUNTAIN	3	3	1	0	0	1
Wyoming	2	2	1	0	0	1
PACIFIC	3	7	3	2	7	3
California	3	6	3	0	7	3
CANADA	4	1	0	0	0	0
EUROPE	17	14	33	22	0	0

One each born in Delaware, Georgia, North Dakota, and Utah.
One employed in Kansas, North Carolina.
One college graduate from Colorado, Oregon, Texas.

24, 1931, 735-757; Social geography, *Social Forces*, 10, 1932, 351-355; Are mineral resources socially detrimental? *Ibid.*, 11, 1932, 51-55; Geography of American notables, *Indiana Univ. Stud.*, 1928 (no. 79), 1-138; Where our notables came from, *Scient. Mo.*, 45, 1937, 172-177; Distribution of the younger starred scientists, especially those in the physical sciences, *Amer. J. Sci.*, 37, 1939, 48-65; The education of the younger starred scientists, *J. Higher Educ.*, 10, 1939, 124-132; Distribution of geologists starred 1903-1937, *Bull. Geol. Soc. Amer.*, (in press).

clusions based upon statistical evidence is much strengthened by correspondence of separate bodies of data, the present study has a significance considerably greater than it would have if it were the only study of its kind. Conclusions which this study alone would not justify, but supports, have some propriety here.

In brief, it appears that most leaders (including distinguished psychologists) come from regions possessing at least a fairly favorable, stimulating climate, a moderate degree of cultural advancement, and many people who are alert, ambitious and persevering. The significance of the quality of the population appears to be so great that effective steps are justified to increase the birthrate of the more capable people. As desirable as relief, medical care, pensions for the aged, etc., may be for the sub-average and average people it seems even more socially desirable to increase the percentage of superior people. The help of the National Youth Administration towards the education of the superior young people is a step in the right direction.

Odin, Cattell, and others who maintain that encouragement ("opportunity") is of prime importance are correct provided unused high ability is abundant. Havelock Ellis and many more recent students of the subject believe, however, that, because of the relative facility with which people of exceptional ability have recently migrated to better opportunities, it is no longer safe to assume that all that is needed is to offer additional opportunities in order to have qualified people promptly appear on the scene to make the most of them. Even if this is still largely so, it will not continue to be true if the dysgenic birthrate long continues. The predominant share of the city in the yield of the younger distinguished American leaders in practically all fields, including agriculture, is ominous when it is realized that almost no city districts but the slums have a birthrate nearly adequate for maintenance.

The great importance of highly stimulating college teachers is so clearly indicated by many data that it appears obvious that such leaders should be widely distributed. Each state university (and as many non-state institutions as possible) should have on their faculty one or more highly enthusiastic research-worker-teacher in each of several great fields of work so as to stimulate various types of young people—some can become enthusiastic over geology but not over psychology!

The evidence indicates that for the doctorate, outstanding leadership and the best of advanced training is highly desirable. Since these can be had in few places, it seems socially desirable that promising students be encouraged to attend one of the few chief centers of the time. In other words, although a widespread distribution of enthusiastic, highly capable research-worker-teachers is highly desirable, it does not seem advantageous that the doctorate be given in numerous institutions. Instead, after receiving the baccalaureate degree and perhaps the master's degree, the promising student should be encouraged and facilitated to attend one of the few best-equipped universities in the land in his subject.

The correlation between the distributions of employment and of birthplaces suggests forcefully that where superior people form a considerable and well-respected fraction of the population not only are relatively many future leaders born, but current leaders are appreciated and employed. Thus, failure to be interested in, and to actively desire, distinguished scholars indicates a serious lack on the part of administrators, trustees, and other influential citizenry.

Indiana University STEPHEN S. VISHER

A Paradox in the Use of the Normal Probability Curve

The fundamental handicap to scientific and social progress lies in a paradox inherent in man's habits.[1] In order to observe effectively, we need principles and techniques to guide our seeing, but, if these are uncritically accepted, they set up resistance to seeing what our habits have prepared us not to see. The paradox is that we are served by our rational formulations and scientific techniques and by the same acts are bound to irrational and non-adaptive principles and techniques that these processes have developed. The aim of this paper is to question subservience to the normal distribution in the quantitative measurement of mental and social traits, and to stimulate the development of statistical techniques applicable to non-normal distributions.

The binding effect of adherence to normal distributions and of techniques which assume its accuracy seems to be attested by our common faith in its validity and by the statistical habits of the profession. Garrett voices the common faith in these words: "Measurements of natural phenomena, as well as measurement of social and mental traits, tend to be distributed symmetrically about their means in proportions which approximate the normal probability distributions."[2] His book is a record of common faith in this generalization, for there is a notable absence of techniques dealing with asymmetrical distributions and *a priori* theories of probability.

It is difficult to reveal a scientist's bias or partiality, but an unbiased or an absolutely impartial scientist rarely, if ever, exists. It may be assumed, however, that bias operates within a limited range and that it probably operates unconsciously, for most scientists display objective measurements, adhere to mathematical and scientific procedures, and assume impersonal attitudes. Bias is, however, to be found in the selection of problems. A preference exists for investigating traits that are unfavorable to predominate loadings, either because of the multiplicity and diverseness of the variables or because of their lack of specificity. Examples of these are found where there is a leaning toward studying traits that are native, or that are general and stylistic. Bias is also indicated by attention to manipulations that influence the normality of distributions by a choice in length of test or step-interval that yields the best fit to the normal curve, and by a restriction of its application to narrow levels of attainment and to samplings that give the desired normality. It appears in the assignment of weights to test-items and, especially, in the use of techniques derived from the normal curve to scale the units. It appears in the general failure to measure and report degrees of goodness of fit to the normal curve.

A motive predisposing to the assumptions of the normal curve lies in the fact that, if the normal hypothesis is accepted, well defined techniques for obtaining quantitatively supported answers are readily available; but, if the assumption of non-normality is made, such techniques await development. For example, coefficients of correlation are available for answering questions of the validity and reliability of tests if normality is assumed, but no such methods exist for the non-normal dis-

[1] This paradox is clearly set forth in the contrasting views of habit revealed in the works of James and Coe. James, W., The Principles of Psychology, 1907, Vol. I, 1-689, Vol. II, 1-704. Coe, G. A., The Motives of Men, 1928, 1-265.

[2] H. E. Garrett, *Statistics in Psychology and Education*, 1938, 106.

tributions. If such techniques are applied to distributions which poorly fit the normal curve, the question may well be raised, whether the deficiency is in the methods or in the assumptions. This question needs to be considered, for partiality exists in places where it often is overlooked. Disillusionment regarding the formulations and impartiality of scientists may be very disconcerting, but the risk of that is potentially less dangerous than the failure to appreciate it.

The question of the adaptiveness of the statistics of normal probability merits serious consideration. The guiding principles of the theory of probability as applied to mental and social measurement are of the *a posteriori* type, *i.e.* the theory can only be utilized with the aid of previously obtained observations of similar phenomena. In the matter of drawing inferences, this type represents a reverse process of the *a priori* theory of probability which rests upon deductive logic and three basic assumptions: (1) the possibilities must be known to be mutually exclusive; (2) they must be known to be equally likely to occur; and (3) the favorable possibilities of frequencies of the phenomena must be known. The normal probability distribution is a graphic representation of the three *a priori* knowns. There seems to be no clear cut reason for expecting that all psychological continuums should satisfy these three assumptions. On the contrary, the logic of the matter is against the principle of equal likelihood. It is implicit in the expression of true, unbiased, or unloaded dice, but why should the ordinary "run of the mill" dice be unbiased in fact as well as in hope? In psychological parlance, the principle of equal likelihood is expected in concepts of nativeness, common environment, and random sampling, but, in the run of the mill products, the fact of biasing or loading is not improbable. True there does exist a fairly large amount of accumulated evidence that obtained distributions approximate the normal symmetry. It seems unwarranted to conclude from this evidence that all psychological distributions must follow the normal pattern.

The assumption of equal likelihood is also quite incompatible with another generalization which is just as widely accepted; namely, that we can figuratively load the dice. We do believe that the efficiency of individuals can be modified by teaching specific skills, and learning may be considered a form of loading. We do believe that efficiency (in part, at least, the product of learning) can be modified by changing incentives, by social approvals and disapprovals, by administrative management, by differences in age, culture, racial stocks, conditions of health and immediate environment. This implies a faith in loading by varying learning, motive, and interest and by manipulating diverse conditions. We frequently find results are significantly modified by such conditions. It would seem difficult to reconcile the idea that loading occurs, with the generalization that we always expect distributions to be normal.

It would seem to be quite an assumption to infer that a curve superimposed on obtained data that is approximately normal after the fashion of *a posteriori* techniques, would also exactly coincide with one that utilized *a priori* theory. Using attitude data and three obtained curves, the writer obtained a satisfactory fit to the normal curve employing chi-square techniques and Fisher's P table when based upon *a posteriori* theory, and clear independence of the normal distribution when the deductive *a priori* probability-theory was made the standard of comparison. This emphasizes the desirability of recognizing that the two types of probability-theory supplement each other, and the need for using both in developing generalizations and drawing inferences.

The frequent non-adaptiveness of the normal probability-hypothesis is revealed by accumulated evidence and the correctives developed to meet its deficiencies. One type of corrective gives us a set of formulae for dealing with skewness or slight departures from symmetrical distributions. In the predictive field, another type of corrective is to certify the qualifications, breadth of instruction, and experience of those who administer and interpret test results. The expert can presumably take the necessary precautions and make the appropriate qualitative corrections for the inappropriateness of the tools and techniques and bring forth valid generalizations for the specific purpose desired. It would seem more desirable to develop the appropriate tools and connect the demand for expertness with efficiency in educing generalizations in such complex fields as counselling, personal selection, and clinical diagnosis. The need for more than mere correction is recognized in the field of social phenomena. Here the J-hypothesis is expected to hold for fairly homogeneous groupings influenced by social conforming pressures.[3] The pattern is a highly asymmetrical or non-modal form, resembling the reverse letter J. In spite of the asymmetrical recognition, the techniques and assumptions of normality are often employed in analyzing such data. The writer's critical attitude was enforced by an unpublished study of attitudes in which all of his 176 distributions were found to be completely independent of chance fluctuations to the normal curve. The technique employed was Pearson's chi-square test for goodness of fit and Fisher's "P" table. There are a few critics of quantitative measurement as practiced. Johnson argues that opinions and attitudes do not stand in a continuum.[4] Certainly, if we must define a continuum as it is customarily allied with the chance hypothesis, one must heartily agree. Johnson's conclusion is forthright, for it is in effect that, if quantitative methods are non-adaptive and do not measure, then use qualitative methods. This solution would be much like recommending that a mechanic cease using cutting tools because his tools are unsuited to his purpose. The saner program would be to develop other tools for cutting or for measuring the continuum.

While we have no felt competence for the development of useful techniques applicable to non-normal and asymmetrical distributions, some of our thinking may stimulate their development. The most hopeful line of attack seems to take its point of departure from the neglected *a priori* probability-theory rather than *a posteriori* development. From the *a priori* normal curve as a point of departure, we should have considerable use for a measure of deviation that would give a numerical description indicative of the amount of loading present in an obtained distribution. Starting from the assumptions of mutual exclusiveness, equal likelihood, and the distribution common to the law of errors, we have considered the following method to have some meaning and usefulness. Superimpose the obtained distribution, over the *a priori* theoretical curve; compute or utilize tables to obtain two chi-square values, one for the obtained distribution from the *a priori* curve, and the other for that giving 1 chance in 100 that the fluctuations can be attributed to chance; then divide the first chi-square value by the second. The deviation value so obtained, if 1, would indicate bare independence of chance as defined by a single

[3] F. H. Allport, The J-curve hypothesis of conforming behavior, *J. Soc. Psychol.*, 5, 1934, 141-183; D. Katz and R. L. Schank, *Social Psychology*, 1938, 35-54.

[4] H. M. Johnson, Pseudo-mathematics in the mental and social sciences, this JOURNAL, 48, 1936, 342-351.

probability in 100 chances; values greater than 1, would give a deviation unit, whose distance is defined by a bare independence being equal to 1. Another measure that would seem desirable would be some indication of the degree of extreme "J-ness" in one direction. Starting again from the theoretical *a priori* probability distribution and assigning positive and negative values to the contrasting mutually exclusive directions, we calculated for the positive and negative ordinates the chi-square contributions which were greater than those of the theoretical chance distribution and obtained the algebraic sum; then divided this by the total chi-square value to obtain the percentage so contributed. In a U-type distribution, we obtained the percentages so contributed by both the positive and negative sums independently. When such percentages are given along with the size of the chi-square value, considerable information can be educed from these numerical descriptions. The problem of finding appropriate scaling units for asymmetrical curves in the favored probability units is desirable. We suggest letting the rater assign the probability-values with reference to *a priori* or deductive probabilities. For this, we have asked the rater to assign a total of 10 probabilities to a psychological continuum. For example, if a continuum is defined as extending from being extremely dominant socially to that of being extremely submissive, the 10 probabilities could be allocated in many possible combinations to that defined continuum. The 10 probabilities might be concentrated at a single point, if that point expressed the rater's behavior as consistent under all circumstances, social environments and conditions; or they might be spread over the entire reportable scale, if such would better indicate the spread and inconsistencies of his behavior. A single numerical value for any possible combination can be developed for a scale such as a seven-point double rating scale. We have utilized the chi-square percentage contributions greater than chance as described above, indicating contrasting directions by appropriate signs. While this device puts the task of probability-scaling on the rater, this seems to be an improvement over making normal curve assumptions where they are contrary to fact. It permits a greater flexibility in reporting behaviors and recognizes the fact of behavioral inconsistencies. In starting from the deductive probabilities, it also avoids subservience to the assumptions of normal distributions and *a posteriori* probability-theory. These measures are included here merely as suggestions rather than as developed or useful techniques. The development of useful and meaningful measures would seem most desirable to foster the advancement of investigations of attitudes, personality traits, and predictive problems in such fields as counseling, personnel selection, clinical dignosis, and perhaps in supra-liminal perceptive studies.

The paradox mentioned in the beginning deepens toward the ridiculous and the tragic when one discovers that those scientists who were most daring in throwing off the fetters of authority by developing quantitative measurement, laboratory controls, objectivity in observations, and academic freedom, have become such worshippers of the normal probability curve that they are bound to its authority and have unconsciously lost the freedom that they earlier obtained at such great effort and cost. It must be in the mingling of faith and distrust that freedom is won and maintained.

Lehigh University JAMES L. GRAHAM

THE EFFECT OF BENZEDRINE SULFATE (AMPHETAMINE SULFATE) ON SATIATION

In a recent paper, satiation was defined by Barmack as "a state of conflict between the tendency to continue and the tendency to get away from a situation which has become unpleasant, principally because of inadequate motivation resulting in inadequate physiological adjustments to it. Accordingly a state of boredom is initiated by *inadequate motivation* during the operation of a task set and results in a tendency to revert back to the *sleep level*."[1] In the attempt to substantiate this hypothesis, Barmack administered 10 mg. of Benzedrine Sulfate (amphetamine sulfate) and equated blank pills to 36 *Ss* in order to determine the effect on the rate and accuracy of adding 6-place numbers for 2 hr.[2] He found that the drug retarded a drop in the rate of addition, and also retarded the development of boredom, sleepiness, and fatigue. Since the accuracy of the performance was not affected, and since the differences in the rate of work were increased with the increased duration of the work period, Barmack believes that the action of Benzedrine (amphetamine) was on the inclination, rather than on the ability to perform repetitive work. It was concluded that the experimental facts substantiate the hypothetical definition of satiation.

The findings were confirmed, in a later experiment,[3] with a dose of 15 mg. of Benzedrine (amphetamine) and 120 min. of activity on the Poffenberger pursuitmeter.

This note attempts to indicate some difficulties inherent in the Barmack definition of satiation, and in the experiment which apparently confirms the hypothesis.

When it is observed that an individual is highly motivated, then with continued activity becomes disinterested, and finally rejects the activity entirely, it is plausible to consider that the motivation has somehow disappeared; that in some way the tissue needs, states of agitation, or other conditions which are the basis of motivation (drive), have become appeased, and then saturated to the point of aversion. This is certainly true in certain forms of satiation which we consider primitive; where the satisfaction of a basic need is involved, and is well illustrated by the work of Katz on hunger and appetite.[4] Barmack adopts just this point, but errs when he generalizes this restricted view to all satiation. He does not consider satiation in the wider sense of a dynamic reaction in the behavioral field. He ignores the findings of Lewin and Karsten who showed that satiation may arise suddenly and leave suddently, *i.e. S* may be satiated at one moment and not in the next.[5] This frequently occurs when the stimulus-field undergoes even a slight reorganization. Under these circumstances it does not seem probable that these motivational states could be resolved, or that a transformation from desire to aversion could be accomplished, in so short a period. In an operational sense, it is known that animals require a relatively long period—within a particular sphere of activity—to satiate a basic need

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[1] J. E. Barmack, The effect of benzedrine sulphate (benzyl methyl carbinamine) upon the report of boredom and other factors, *J. Psychol.*, 5, 1938, 125. (Italics ours.)

[2] *Ibid.*

[3] Unpublished experiment by Dr. Barmack. Personal letter to the writer.

[4] D. Katz, *Animals and Men*, 1937, 1-263.

[5] K. Lewin and A. Karsten, Untersuchungen zur Handlungs- und Affektpsychologie: V. Psychische Sättigung, *Psychol. Forsch.*, 10, 1928, 142-254.

(*e.g.* hunger) and if this satiation were to occur in an instant, it would probably be due to mediating factors and not to the physiological, organic, or vital activities which we think of as the basis of motivation. These mediating factors, which in part determine the continuation or rejection of the activity, seem to involve a higher level of behavior than the physiological, and are of great importance to a theory of satiation. By this we mean to indicate that satiation involves more than a reduction of motivation during the task. There seems to exist a confusion between the recognition of the subjective aspects of fatigue and that of satiation. It is fundamental that a one-to-one correspondence between these basic states and satiation does not seem to hold. Katz has made this the basis of his two component theory for he finds that hunger, and the satisfaction of that hunger, is to a surprisingly great extent independent of the physical state of the organism and largely dependent upon the external circumstances of the event.[6]

If we deny a strict correspondence between motivational states and satiation, then we cannot accept the hypothesis that satiation is a "tendency to revert back to the *sleep level* arising through a *loss of motivation*."[7] The evidence to support this contention seems insufficient—in addition to that presented—on the following grounds.

(1) The criterion of 'sleepiness' was *S*'s report on a 10-point scale, ranging from 1 and 2 (very sleepy), 3 (sleepy), 4, 5, 6 (indifferent), to 7 and 8 (wide awake), and 9 and 10 (very wide awake). We doubt whether any *S* has the ability to discriminate accurately over a 10-point scale. It was found in one of our experiments that responses to a 5-point scale were not sufficiently reliable to be the sole criterion of satiation. Barmack's data seem to indicate that the greatest difference between the two groups on the sleep-wake scale was no more than 1 point on a 10-point scale. The probable error of the scale is such that the small obtained differences might easily be overcome or reversed. A criterion of satiation should involve—in addition to a subjective report—non-resumption of activity, characteristic variability in performance, and the presence of "creative substitution," *e.g.* day-dreaming.

(2) As is well known, the requisite for sleep, or a sleep-like state, is the exclusion of sensory stimulation.[8] An abundance of stimuli in the field, as would be required to satiate an individual, would reduce the tendency to go to sleep.

In certain instances where *S* becomes adapted to identical repeated stimuli, he may restrict the field of attention (as in hypnosis), and where the initial tendency is towards a drowsy state, this may facilitate sleep. Satiation is, however, not restricted to repetitive stimuli—and heterogeneous stimulus-fields tend to a waking state. We have no evidence that those individuals who tend to sleep during repetitive activity, would not do likewise in activities upon which they do not become satiated.

(3) Postulating a 'sleep-level' adds nothing to the explanation of satiation beyond the already apparent fact of the conflict between staying at the task and its rejection; that variegated accompaniments of this conflict appear which may in certain instances be postural, attentive, psychical, visceral, and fantastic.

The pharmaceutical action of Benzedrine Sulfate (amphetamine sulfate) as a relief

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[6] Katz, *op. cit.*, 159 ff.
[7] Barmack, *op. cit.*, 125. (Italics ours.)
[8] Katz, *op. cit.*, 34.

for exhaustion and for an increased capacity for work,[9] as a vaso-constrictor,[10] as a general activator of behavior,[11] has been demonstrated clinically and experimentally. It may be said in general to produce a restless hyperactive state with increased tensions and tendencies to activities. Skinner and Heron report that "the effect of either drug [Benzedrine or caffeine] is to cause the organism to release energy at a higher than normal rate whether in a state of inhibition or not. The surplus energy is used by the animal in doing what it is accustomed to doing in the situation."[12] It would seem, then, that the action of Benzedrine (or caffeine, ephedrine,[13] and even a stimulus which elicits an intense emotional response—*e.g.* fright) serves to increase the available energy and the extensity and intensity of the tension systems. It is thus possible for *S* to persist longer at the task because of the greater potentiality of these systems. Any drug or stimulus of sympatho-mimetic character thus performs this function to a greater or less degree. This, of course, does not mean that the previously discussed motivational states are necessarily increased under such stimulation, for it is easily shown that such stimuli do not actually increase the desire for activity. In addition, it may be that an increased "vitality" of these *S*s also introduces a reorganization of the attitude toward the rapidly declining "demand value" of the task.

In his earlier studies,[14] Barmack seems to recognize the more than motivational character of satiation by recognizing a "feeling of boredom" and an "attitude of boredom," although the distinction is not strictly maintained throughout the paper. No evidence of more than the primitive aspects of satiation is apparent in the later work.

This note is in part directed to just this recognition of the confusion between the motivational and physiological aspects of satiation on the one hand, and the psychical on the other. It does not posit a strict dualism between motivation and satiation (for this is obviously incorrect), but attempts to indicate that all aspects of satiation cannot be explained on the grounds of basic motivational states, and tendencies to revert to a sleep level.

It is quite probable that Barmack would have found somewhat different results had the satiation activity been more intrinsic to every *S* than adding 6-place numbers.[15] His *S*s, because of strong extrinsic incentives, were probably forced to exhaustion, which is not the same as satiation, and may even have interfered with satiation.

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[9] L. Zieve, The effect of benzedrine on activity, *Psychol. Rec.*, 1, 1937, 393-396.
[10] S. A. Peoples and E. Guttman, Hypertension produced with benzedrine, *Lancet*, 1, 1936, 1107-1109.
[11] L. V. Searle and C. W. Brown, The effect of subcutaneous injections of benzedrine sulphate on the activity of white rats, *J. Exper. Psychol.*, 22, 1938, 480-490; W. J. McNamara, and R. E. Miller, The effect of benzedrine sulphate on mental work, *Psychol. Rec.*, 1, 1937, 78-84.
[12] B. F. Skinner and W. T. Heron, The effects of caffeine and benzedrine on conditioning, *Psychol. Rec.*, 1, 1937, 339-346, esp. 346.
[13] In an unpublished experiment, Barmack found similar effects with ephedrine hydrochloride—although less intense than for benzedrine sulphate.
[14] Barmack, Boredom and other factors in the physiology of mental effort, *Archiv. Psychol.*, 31, 1937, (no. 218), 1-83.
[15] I am indebted to F. Nowell Jones for this suggestion.

Individual Psychology and Psychological Varieties

The first term in this title has a long tradition. Individual Psychology derives in part from discrepant items in reports (formerly called 'introspections') of the several observers or subjects serving in a given research. For many years it was common for experimental expositions to conclude with a section on 'individual differences.' Here were added to the generalized results a comparative survey of the several protocols. Another origin lay in measures of variability from central tendency. Again, the testing methods placed their emphasis from the first upon the description (usually in quantitative terms) of the individual's output and upon differences among individuals tested.

Gradually there came to be separated off from the 'general' studies a division of the subject which was concerned, as Binet and Henri explained in 1896,[1] with the determination of those characteristics of mental processes which may vary from individual to individual and which are presumably grouped or constellated in a given organism according to a certain pattern. In 1900 Stern brought together a large mass of this 'differential' material in an important little book which opened with the prophetic exclamation "Individualität, Problem des zwanzigsten Jahrhunderts."[2] Stern stressed the experimental method, as Binet had favored the methods of test and of the questionary, Cattell (closely following Francis Galton) the methods and aims of a gross mensural anthropology, and Kraepelin the Wundtian pattern as applied to the diagnosis of the pathological subject.

In this country Jastrow had assembled apparatus and carried on a testing bureau at the Columbian Exposition of 1893 and the American Psychological Association had appointed, as early as 1895, a committee (Cattell, Baldwin, Jastrow, Sanford and Witmer) to report on "the collection of mental and physical statistics." A series of tests of mental ability was later recommended to the American laboratories and discussed at length at the Ithaca meeting of 1897. The wide program in Individual Psychology which thereupon developed in the United States fused French, German and English views and methods. The subject was ripe for a wider exploitation when pressure to test the individual child was exerted from education and was exerted later from business, industry, and medicine to test the adult. As the pressure-groups were both strong and insistent upon speedy results, the experimental methods of the laboratory were rapidly mutilated or entirely discarded for the

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[1] A. Binet and V. Henri, La psychologie individuelle, *Année psychol.*, 2, 1896, 411-465.

[2] L. W. Stern, *Ueber Psychologie der individuellen Differenzen* (*Ideen zu einer 'differentiellen Psychologie'*), 1900, 1-146. Upon establishing, as he avers, a new "Wissenszweig mit eigenen, genau zu formulierenden Aufgaben, Zielen und Schranken," Stern proceeds to expound the principles and the facts of this new discipline. Since his study includes differences between and among peoples, social states, races, genera, and so on, he will not regard it as a 'psychology of personality.' It is designed to include "alle psychischen Differenzierungsmöglichkeiten überhaupt" (p. 4), to include all sorts of differentiable varieties of a psychological nature. In his more compendious volume of 1911, *Die differentielle Psychologie in ihren methodischen Grundlagen*, Stern more clearly distinguishes between the study of varieties (*Variationslehre*) and the biographical depiction of the individual (*Psychographie*). Most of the book is devoted to the former, *i.e.* to the study of varieties.

quicker and easier methods of test, a change which inevitably brought in its wake the application of statistical devices for refining and interpreting crude and raw data from the testing room.[8]

It is noteworthy that, in spite of the tendency for the individual thing (whether measure, trait, organism or 'person') to disappear under the methods of variation, correlation and factoring, the important distinction drawn by Stern and others as between *the strictly biographical* (psychogram, profile, autobiography, analysand, and the like) and *the variational in kind* has been widely ignored—the term 'individual psychology' being illogically used to cover both. This fact is the more striking because the biographical methods themselves—as well as the others—are in very large measure taxonomic, using the individual only as a representative of class, genus, type or grade. Whether, under the present strong domination from practice, most psychologists would join other descriptive sciences in a formal declaration that the strictly individual and unique is never itself the proper object of inquiry in a scientific context is doubtful. Practical responsibility for cases, candidates, wards, and inmates tends to persuade us that these 'persons' offer primary material for study. Besides, ambiguous terms always have their convenient uses. The adjective 'individual' in our title may mean either a unique sample, this thing, quality, or attribute, the unanalyzed organism, the total organism, the organism with all aspects but one ignored, the representative of a kind, a 'type,' or that member of a group who is just now undergoing test and classification. Something of this kind lurks under the current popularity of all sorts of reference to *personality*. The trend of the times may possibly stand related to the threat of individuality from dictators and despotic states; but a more obvious reference is to a recent diversion from the social group to the socialized individual, especially the defective, the aberrant, and the pathological individual.

While all the sciences of living things deal inspectively and descriptively with the individual organism, there seems to be no reason why the psychologist should concern himself any more or less with the single organism *qua* individual than the physiologist or the anatomist does. In all the sciences of life the organism is either a *fair sample* or it is a *variant* of a certain degree or a certain kind within its class. The first context is that of general psychology; the second (the taxonomic) context implies that the organism is representative of some psychological variety or that it takes its place in a variational display. A strictly biographical or personal account ('individual' in a social sense) would seem to find its place in some such subject as clinical medicine, education, economics, or daily living, where it would receive its valuation according to the estimates of its extra-psychological setting. Such a clarification as is here intended is obviously needed not only in the confused and badly designated subject of 'individual' study, but also in social, educational, genetic, and other special psychologies.

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[8] For a discussion of the large part played by this individualizing tendency in the two Great Invasions of the subject during the years 1900-1925, see the writer's The nature and uses of experiment in psychology, this JOURNAL, 50, 1937, 452-469. The fact that the invaders commonly brought for solution a child, a case, a candidate for a position, an alleged medium, or a suspected criminal easily enlarged the belief that it was also a function of psychology to deal with and to resolve *the individual instance*.

The Fifth Princeton Meeting of the Society of Experimental Psychologists

The thirty-fifth annual meeting of the Society of Experimental Psychologists was held at Princeton University on Monday and Tuesday, April 3 and 4, 1939, under the chairmanship of Professor H. S. Langfeld. It was attended by 27 members from 19 institutions, as follows: Brown (Graham, Hunter, Schlosberg); Chicago (Klüver); Columbia (Poffenberger); Cornell (Dallenbach, Weld); Harvard (Beebe-Center, Boring); Montreal Neurological Institute (Jasper); New School Social Research (Wertheimer); New York Psychiatric Institute (Landis); Pennsylvania (Fernberger, Wendt); Princeton (Bray, Langfeld, Wever); Rochester (Culler); Rutgers (Pratt); Smith (Koffka); Swarthmore (Köhler); Tufts (Carmichael); Vassar (Lanier); Washington (Jacobsen); Wesleyan (McGeoch); Yale (Hull, Marquis). That is the widest representation and the largest attendance since the reorganization of the Society in 1929.

After a short business meeting Monday morning, during which Professor S. W. Fernberger of the University of Pennsylvania was elected Chairman of the Society for the coming year, the rest of the sessions were devoted to reports of work in progress or recently completed.

Monday evening, at a banquet given to the Society by Princeton University, Dean L. P. Eisenhart of the Princeton Graduate School extended the University's greetings and welcome. After a response by the Chairman of the Society, Professor Langfeld, the 1939 award of the Howard Crosby Warren medal for outstanding research in experimental psychology was made to Dr. Carlyle Jacobsen "for his description of the function of the frontal lobes of the brain."

The 1940 meeting of the Society is to be held at the University of Pennsylvania. The tentative dates are March 25 and 26.

K.M.D.

William McDougall: 1871–1938

Professor William McDougall died in Durham, North Carolina, on November 28, 1938, in his sixty-seventh year. For some months he had borne with fortitude the knowledge that a cancer would soon end his life.

Probably no other man had better preparation for the study of human nature than McDougall. Born in Lancashire, England, on June 22, 1871, the son of a chemical manufacturer, he was educated in a private school until at the age of fourteen he was sent for a year to the *Realgymnasium* at Weimar. He entered Owens College, Manchester, thereafter and studied for four years, in the latter part of this period working chiefly in biology and geology. In the fall of 1890, having won a scholarship at St. John's College, Cambridge, he began another four-year period, in the last two years of which he specialized in physiology, anatomy, and anthropology. Then followed a medical course at St. Thomas' Hospital, London, and a period spent with Haddon and Rivers on the Cambridge Expedition to Torres Straits and with Dr. Charles Hose among the headhunters of Borneo. In the Straits he helped Rivers investigate the sensory capacities of the natives. There followed still another year of preparation, which he spent a year at Göttingen attending the lectures of G. E. Müller on psychophysics and memory and working on vision.

In 1900 he was appointed Lecturer in Psychology at University College, London. During his tenure of this post he conducted experiments on vision at his country home in Surrey. In 1904, McDougall succeeded G. F. Stout as Wilde Reader in Mental Philosophy at Oxford. Here his work was carried out with difficulty, since Oxford has never given much recognition to psychology. The scientists took him for a philosopher, and the philosophers thought that he was trying to work in an impossible and empty science. He had a few advanced students (W. Brown, Cyril Burt, and H. B. English among others), and Professor Gotch allowed some of the rooms in his physiological laboratory to be used for research. The founder of the Wilde readership objected to experimentation and tried to have McDougall ousted, but in 1912 his position was strengthened by his election to the Royal Society and to a fellowship in Corpus Christi College. In 1913 he made his first trip to America, in the company of Dr. Osler of Oxford, to make an address at the opening of the Phipps Clinic in Baltimore. When the World War broke out, McDougall enlisted as a private in the French army and drove an ambulance. He later offered his services to the medical corps of the British army and was made a major. His work was with military cases of mental disorder and was recognized after the war by his election as president of the psychiatric section of the Royal Society of Medicine. He was called to Harvard University in 1920 and stayed there until he accepted the offer of a professorship at Duke University in 1927.

The scope of McDougall's writings and his intellectual interests is comparable only to that of Wundt and James. He employed almost all methods of psychological investigation, and he made contributions to almost all of the principal fields of the subject. Let us consider some of his dominant interests, which by their variety exhibit the richness of his mind.

Experimental and physiological psychology. Between 1901 and the World War, McDougall published about twenty papers, the most important of which dealt with the following topics: Thomas Young's theory of color vision, physiological factors

of the attention process, sensations excited by a single momentary stimulation of the eye, variation of visual intensity with duration of stimulus, the relation between corresponding points of the two retinas, and learning and retention with respect to Bergson's distinction between habit memory and pure memory. His work shows a strong interest in inferring from sensory and perceptual phenomena the nature of processes in the brain. In 1903 he published his drainage theory of inhibition, which received favorable attention from Sherrington. Although this theory, like the idea that learning involves a decrease of synaptic resistance, belongs to that class of neurological theories for which there is considerable plausibility but little or no direct physiological evidence, its author, with characteristic tenacity, never surrendered it. As late as 1929 he claimed that all of Pavlov's phenomena of inhibition were in harmony with the drainage theory. In 1905 the small but extremely influential *Physiological Psychology* appeared, in which psychology was defined as "the positive science of the conduct of living creatures."

Hormic psychology and the theory of instincts. For a decade McDougall's *An Introduction to Social Psychology* (1908) was undoubtedly more successful and influential than any other psychological work. Although written for the general public, it was widely used for many years as a textbook. Largely a synthesis of the views of other writers, it was nevertheless a brilliant work of systematization. In this book McDougall sought to lay a firm foundation for the social sciences to replace the *ad hoc* psychologies previously concocted by many writers in this field. This firm foundation he believed to be the theory of instincts and their derivatives, the sentiments. In his *Outline of Psychology* (1923) he carried this theory farther and named it, after a suggestion of Percy Nunn, the hormic theory. This theory states that purposive striving is the most fundamental category of psychology, and that the springs of purposive activity are the instincts. The instincts are inborn dispositions, which cannot be defined by reference either to definite stimuli or to bodily movements, but only by the kinds of change in the situation which their activity brings about or, when introspection is possible, by the specific accompanying emotional quality. The ascription of a specific emotion to each of the human instincts is one of the most original but at the same time one of the most questionable points in McDougall's system. One of the arguments for the existence of human instincts upon which McDougall most strongly insisted was that from evolutionary continuity; this argument is especially prominent in the *Outline,* where the instincts of both men and animals are discussed in the same sections. His tendency to bring human and animal behavior so close together was somewhat checked in 1933 by his introduction of the term "propensities" in place of "human instincts," a verbal change which called attention to the difference between the relatively fixed behavior to animals and the very variable manifestations of human instinctive activity. These pronounced hereditarian views of McDougall may be illustrated by his idea that in gregarious animals and human beings certain instincts may be aroused, independently of any learning, by the sight or sound of the same instinctive activity on the part of another individual.

One of the central doctrines of McDougall's hormic psychology is that all behavior is purposive and bears six objective marks which distinguish it from mechanically produced movements. Reflexes are not to be confused with "behavior," for they do not show these marks of purposiveness; they are, like habits, devoid of any driving force and may serve, as mere "motor mechanisms," any instinct.

Purposive striving in McDougall's system is said to be accompanied by a unique experience, which cannot be reduced to sensations of muscular contraction. Perception and memory are held to be governed to a large extent by conation. All instinctive striving in men and animals involves some degree of foresight and insight however rudimentary. In *The Energies of Men* (1933) McDougall gave both these factors great prominence, especially in his discussion of animal learning—a section based partly upon animal experiments which he had made in collaboration with his son.

McDougall's theory of sentiments he took, with some modifications, from the work of G. F. Shand. He held that the sentiments are the functional units of mental life; each sentiment is an organization of all the instinctive and cognitive dispositions relating to a particular object. He called the theory of the sentiments "a systematic sketch of the structure of character and a theory of its development from native tendencies under the moulding process of social traditions."

Social psychology. McDougall's strictly social psychology began with *The Group Mind* (1920); his work of 1908 had been largely the groundwork in individual psychology which was to be applied to social phenomena in his later writings. His book of 1920 discussed the concept of the group mind (not to be confused with the rejected concept of group consciousness), the behavior of crowds and organized groups, and the national mind and character. *The Group Mind* was intended to form one part of a *magnum opus,* but McDougall never completed the project, since this part was so unfavorably received. Undoubtedly the book was handicapped by its title, which lent itself to misunderstanding. The verdict of critics was that this work was no more scientific than many others, not written by men of science, on the same subjects.

McDougall developed the theory of innate mental differences between races in a series of Lowell lectures, published under the title *Is America Safe for Democracy?* (1921). This book belongs to the period in which the results of mental tests, especially the army tests given to different racial and national groups, were uncritically accepted as revealers of "innate" intelligence. McDougall used these data as evidence of the innate inferiority of the negro race and for the hereditary origin of intellectual differences found among children from different social strata. He also attempted to show that there were certain important temperamental differences among the three races of Europe which were reflected in various social and cultural phenomena. Later in his autobiography (1930) he said that he did not know that the American people were sensitive about the racial question and that this book is largely responsible for a hostile attitude toward him on the part of the American press.

The mind-body problem. In *Body and Mind: A History and a Defence of Animism* (1911) and its sequel, *Modern Materialism and Emergent Evolution* (1929), McDougall defended the theories of psychophysical interaction and vitalism. Mechanical principles he held to be inadequate to explain purposive activity, organic evolution, morphogenesis, heredity, vital self-regulation, and memory. Furthermore, he thought that the correlation between brain processes and mental processes cannot be carried very far, in fact that there are no complete brain correlates for memory, the unity of consciousness, meanings, or intelligent purposive activity. In the later book he vigorously criticized the efforts of H. C. Warren and R. B. Perry to reduce purposive activity to mechanical activity or to obliterate the distinction

between the two. The value of *Body and Mind* as a comprehensive summary of the psychophysical problem and of its sequel as a critique of emergent evolution will remain undiminished for many years to come.

Abnormal psychology. In the field of abnormal psychology McDougall wished to be a mediator between academic and medical psychology. He considered himself a member of a group led by Morton Prince and W. H. R. Rivers. Both of these men had been influenced by McDougall's writings; in fact, McDougall claimed to have converted Prince to hormic psychology. McDougall based his *Outline of Abnormal Psychology* (1926) partly on his military experience. The book is important for its systematic discussion of mental disorders, which made much use of the concepts of instinct, repression, conflict, and dissociation; for its treatment of hypnosis; and for its criticism of Freudian and Jungian psychology. In the last two chapters, McDougall yielded to his speculative tendencies and presented his "monadic theory" of personality.

His criticism of psychoanalysis in this book and *Psychoanalysis and Social Psychology* (1935) is probably the most thoroughgoing and competent that has been made up to the present time by either a medical or an academic psychologist. McDougall preferred not to attack the application of psychoanalysis to individual cases; he chose rather to point out its internal inconsistencies and to criticize its application to other fields, especially social psychology. He was queerly ambivalent toward Freud. After having written in 1920 that he found French writers on abnormal psychology, especially Janet, more valuable to him than those of the German and Austrian school, he said in 1926, "I believe that Professor Freud has done more for the advancement of psychology than any other student since Aristotle." Although he never receded from this opinion, it would be hard to find very much in Freudian psychology of which he approved except its hormic quality.

The Lamarckian experiment. McDougall's belief in the inheritance of acquired characters, which he had expressed in many of his works, led him to begin in 1920 what he has himself called "a fool's experiment" on the transmission of the effects of training in rats. The experiment has now gone on for at least 13 years and 37 generations. The descendants of the trained animals, which learned to escape from a tank of water, made decidedly fewer errors in the latter generations than a control group. Although there has been criticism, principally from Crew of Edinburgh, McDougall believed that all objections could be answered and that he had proved the Lamarckian principle.

Nor was McDougall's versatility limited by even these six fields. As early as 1896 he published a theory of muscular contraction. He contributed several papers to anthropological journals and collaborated with Dr. Hose in 1912 on *The Pagan Tribes of Borneo.* He wrote extensively on topics relating to ethics and world politics. Eugenics and psychic research he called his hobbies; in fact, psychic research became a major interest of his last years, when he actively supported Rhine's work on extra-sensory perception, accepting the results at their face value.

To the last McDougall was a vigorous and indeed an unexcelled polemist. His polemic abilities were a natural outgrowth of his championing a cluster of unpopular causes; he was necessarily belligerent in the face of opposition on so many fronts. Many years ago he decided to write his controversial articles with

utter frankness, and he lived up to this policy. His enthusiasm sometimes produced clever phrases which antagonized some readers (he called behaviorism "a most misshapen and beggarly dwarf" and said that parallelism "survives merely as a historical curiosity still hugged by a few intellectual tortoises"), but his informal oral discussions were marked by mildness and even diffidence. Among his last polemic essays were a devastating criticism of Pareto and a series of articles on Gestalt psychology.

McDougall lived and thought in the tradition of British psychology after the decline of associationism. He has said that he was a disciple of no one but Stout and James. His indebtedness to Stout is perhaps not as well known in America as it should be. His system of psychology in its main features is an extension of Stout's psychology, free from certain doctrines which he considered errors, with greater emphasis on hormic forces, and in a less austere literary style. From James, McDougall got a belief in the pragmatic criterion of truth and a great deal of personal inspiration.

A feeling of isolation, due to the almost unanimous rejection by American psychologists of his hormic theory and especially of his views on human instincts, burdened his last years. He deplored the chaotic state of psychology and became skeptical of its present and future progress and of the value of his own life-work, saying in 1935, in spite of his manifest achievements, "Even now, after some forty-five years of sustained effort, I am not sure that I have made any progress, have learnt anything of human nature."[1]

The Rice Institute FRANK A. PATTIE, JR.

[1] The autobiography of McDougall, in *A History of Psychology in Autobiography*, 1, 1930, 191-223, edited by Carl Murchison, which has been consulted in the preparation of this sketch, is candid and fascinating. It is accompanied by a photograph of McDougall in army uniform. A more recent picture appears in *Human Affairs*, edited by R. B. Cattell and others, 1937. Neither is a very good portrait of McDougall, who was an exceptionally handsome man.

Otto Klemm: 1884-1939

Otto Klemm was born in Leipzig on March 8, 1884, and died there on January 5, 1939. Almost all the important events of his life center about the University of his native city. From 1903 to 1905 he studied at Munich where Lipps turned him in the direction of psychology. Upon receiving his doctorate at Leipzig in 1906, he was offered an assistantship by Wundt which he held until he wrote his *Habilitationsschrift* and became *Privatdozent* in 1909. In 1923 Klemm became *planmässiger ausserordentlicher* Professor of Applied Psychology and Experimental Pedagogy, a post which he held at the time of his death.

In the *vita* prepared for the third volume of the *History of Psychology in Autobiography*, Klemm tells how he became a psychologist.[1] During early adolescence he conceived the "fantastic" notion that trees and buildings always came into being just as he passed them. His first scientific interest appears to have been astronomy, for at the early age of fifteen years he succeeded in publishing a paper on sunspots in a technical journal; the "need to philosophize," however, led him to Lipps' seminar where he was struck with the small collection of apparatus for experimental psychology which stood unused and somewhat dust-covered in a corner of the room. A happy convergence of interests took place and led naturally to his first experiment with the complication-pendulum. Later he spent some time in Rome and Naples writing a prize essay on Vico as a social psychologist, out of which his short but highly successful *History of Psychology* seems to have grown. This book was translated for the American audience by Wilm and Pintner; later editions in Japanese, Spanish, and Italian won him an international reputation. His own translation into German of Titchener's *Textbook* was made during this period.

But Klemm loved the laboratory as well as the library, and his perceptual papers in the pre-war volumes of the *Psychologische Studien* reveal the careful research specialist. His investigation of sensory localization under conditions of disparate auxiliary stimulation demonstrated the law of spatial inducton and showed that localization is affected by the contemporaneousness of minor accompanying stimuli. In a *Mitarbeit* with G. F. Arps (later Dean at Ohio State University) on attention and rhythm, Klemm found that sensitivity to the accented part of a dactyl is greater than to the unaccented. He later discovered that there is a strictly monotic sort of localization and anticipated P. T. Young's famous criss-cross experiment reversing the normal sound paths. His first introduction to applied psychology came when he employed these techniques in training artillerists during the War.

Following the expansion of the activities of the Leipzig Institute under Krueger's directorship, Klemm engaged in a miscellaneous variety of researches and professional duties. He served as a consultant in bureau work for courts, prisons, agriculture, textile factories, sports, and guidance at the same time that he continued his inquiries on illusions and perception. He was the active bibliographical editor for the *Archiv für die gesamte Psychologie* and the *Zeitschrift für Psychologie*, a leading official of the *Deutsche Gesellschaft für Psychologie*, and, after the reorganization of the scientific publications in Germany on an "Aryan" basis, the senior editor of the *Zeitschrift für angewandte Psychologie*.

In his theoretical outlook, Klemm was a Neo-Kantian and a thoroughgoing advocate of the *Ganzheit* position favored by the Leipzig school as distinct from the Gestalt approach of the Berlin-Frankfurt "physicalists." In the paper he read before

[1] O. Klemm, *A History of Psychology in Autobiography*, 3, 1936, 153-180.

the International Congress at Yale in 1929 he held that all experience of meaning is an awareness of belonging to a whole, and in the contribution to the Jubilee volume of this JOURNAL (50, 1937, 157-165) the phrase "Glied zu sein eines Ganzes" appears with monotonous and insistent regularity. He adopted a psychology of types in dealing with educational and other "practical" problems, and, although he surmised that psychologists of the future would be less satisfied with "integralism" than he was, for one whose youth had been gravely disturbed by conflicting theories no other attitude was half so congenial.

Modesty was Klemm's outstanding trait; he was never the pompous overlord to younger students, but always like one of them. A tall, thin, sunburned figure, he engaged in vigorous sports as long as he lived. His writing has a wistful quality; it suggests that what he brought to expression and completion was only a sample of what he had actually lived. He made no pretensions to eminence and suspected that no one else thought of him as a major figure; his was a thoroughly honest mind with a mystical and ethical bent. Nowhere is the mood of the man better revealed than in the few lines with which he ended his autobiography: "How about the answers to those riddles of the Sphinx which ensnared the boy of twenty? The hands of the mature man of today no longer reach for the unattainable; they grasp more firmly that which has become spiritually his own. But his eyes are still fixed upon the stars, which are just as far away now as they were of yore."

Columbia University GEORGE W. HARTMANN

THE CORRECTION OF AN OMISSION IN *Shape as a Function of the Vector-Field*

In my article, *Shape as a Function of the Vector-Field*,[1] Fig. 1, p. 35 of this volume of the JOURNAL, shows the geometrical fields used in the experiments and was intended to show the positions of cohesive equilibrium for the fields, but the

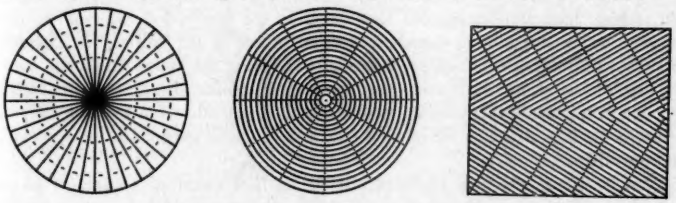

FIG. 1. GEOMETRICAL FIELDS USED IN THE EXPERIMENTS WITH THEIR RESPECTIVE POSITIONS OF COHESIVE EQUILIBRIUM

A, *B* and *C* are the fields used. The dashed lines, which were omitted in the original figure, show the positions of cohesive equilibrium.

lines showing these positions were omitted from the drawings. A corrected figure accompanies this note. In it the fields are reproduced with their respective positions of cohesive equilibrium indicated by the dashed lines. These positions, which Fig. 1 intended to show, are referred to specifically in the article on p. 36, line 1 and footnote 12, and on p, 43, line 3.

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WILLIAM D. ORBISON

[1] W. D. Orbison, Shape as a function of the vector-field, this JOURNAL, 52, 1939, 31-45.

BOOK REVIEWS

Psychology and Religion. By CARL GUSTAV JUNG. New Haven, Yale University Press, 1938. Pp. 131.

In this volume, comprising three lectures delivered in Yale University, the distinguished author gives the experiences of certain patients of his, and what he believes to be their significance for psychology and religion. These particular experiences are interpreted in a context of hundreds of others which have come to his knowledge as a specialist in nervous and mental disease. The book is of important interest in its substance, but is at times ill-formed, and to reproduce accurately its chief findings is no easy task. Until later, let me be strictly an expositor, using to a large extent the author's own words. Criticism may come after a patient attempt at understanding.

His intention is "to give a few glimpses, at least, of the way in which practical psychology becomes confronted with the problem of religion" (p. 2); one of the patients here described has a cancer-obsession which in Jung's judgment, expresses a profound discord between the patient's religious impulses and other promptings of his. Jung aims to deal with his data as a scientific psychologist who finds it "indubitable that the real causes of neuroses are psychological" (p. 10); for he has seen, to give but one of many examples, "a case of hysterical fever, with a temperature as high as 102°, which was cured in a few minutes by a confession of the psychological cause" (10). For Jung, "it is an almost ridiculous prejudice to assume that existence can only be physical;" in fact "the only form of existence we know immediately is psychic;" physical existence is merely an inference from this (11).

What are, more distinctly, the observations on which Jung bases his present conclusions? Along with hundreds of others which are merely referred to, three dreams by a particular patient are given in some detail, in which there is religious imagery and feeling. Jung ascribes especial significance to the *fourfoldness* in some of this imagery, and to the feeling connected with it. In one of the dreams, for example, there is a solemn house which people enter, and within the house "are many burning candles arranged so as to form four pyramid-like points" (42). This, the author connects with many other dreams reported to him and others, in evidence for his belief in the importance of the number four as a religious symbol for these patients and for others.

Now the four, which in the particular dream here described appears as an arrangement of lights, appears in other dreams in other forms, such as a town square, a square garden, and so on (79). In a way not clear to the reviewer, Jung seems to include the circle, globe, clock, wheel, and much else, among the dream-images which display fourness, quaternity, and its religious meaning (65). "The four symbolizes the parts, qualities and aspects of the One" (71). The author connects the symbols in his patients' dreams with like symbols in the Middle Ages, Greek antiquity, and more, which signify a world-creative deity (71).

Psychologists generally will be less interested in this than in Jung's theory of the origin of such images and their freight of meaning. For he holds that "certain ideas exist almost everywhere and at all times and they can even spontaneously create themselves quite apart from migration and tradition. They are not made by the individual, but they rather happen—they even force themselves upon the individual's consciousness" (4). Elsewhere he speaks of "forms or images of a collective nature which

occur practically all over the earth" (63), which "presumably start from the archetypal patterns of the human mind which are not only transmitted by tradition and migration but also by heredity" (63 f.). Or again, "It seems that" his patients' "unconscious mind has worked along the same line of thought which has manifested itself, time and again, within the last two thousand years" (111). Accordingly Jung assumes "a certain unconscious condition carried on by biological inheritance,"—not an inheritance of "representations," but of "something like a possibility of regenerating the same or at least similar ideas." Such a possibility he calls "archetype" and regards it as something both mental and cerebral (112).

In this attempt to explain the origin of certain ideas, we have also touched upon what he offers about religion; for the ideas and images in the forefront of his discussion are religious. Jung is explicit, however, in denying that such ideas—of the One, of world-creative divinity—are proof of the existence of such objects. They and their dream-contexts, he believes, show the untoward effect of repressing what is religious; and health requires that this living part of the psyche be restored to consciousness and reconciled with it (99).

Religion, as Jung here depicts it, is not mere idea, symbol, dogma, and ritual. It is an attitude peculiar to a consciousness which has undergone a particular kind of experience (5 f.)—an experience which to some may be so disturbing, so distressing that creed and the rest are resorted to as a screen and defense against the experience itself (52 f.). To others, however, the immediate experience is welcome and beneficent; it is characterized by the highest appreciation, and it means everything to the one who has it (75); he "possesses the great treasure of a thing that has provided him with a source of life, meaning and beauty, and that has given a new splendor to the world and to mankind" (113).

All the matters touched thus far are constantly connected by Jung with the unconscious mind. What he means by the unconscious must, as for other meanings of his, be compacted of scattered passages.

"As a matter of fact the concept of the unconscious mind," he tells us, "is a mere assumption" (47). It is "the Unknown" (21). Yet much, it would appear, is known about it; for dreams are its voice (21), and both psychoses and dreams reveal the roots of a patient's obsession, conflicts, and complexes, quite as do the association tests (25). Thus associations, too, reveal the unconscious.

From dreams, associations and other sources the Unknown, the concept assumed, can be described with fair fullness and confidence. The unconscious is "an autonomous power" (17); it has "dangerous tendencies" (21); the male unconscious is different from the female unconscious (34); it is "capable at times of assuming an intelligence and purposiveness which are superior to actual conscious insight" (45). Dreams which, as was said above, are the voice of the Unknown, are elsewhere said to be probably "visible links in a chain of unconscious events" (38). Dreams, it would thus seem, are not mere reports about the unconscious; they probably are parts of the unconscious which have become visible to us.

Conscious and unconscious, it would appear, have contents in common or which pass from the one to the other. Contents may be conscious or may be repressed or isolated from consciousness (93); again, many unconscious contents may seem to be the remnants of historical mental conditions which were conscious several centuries earlier (66).

The relation between the unconscious and the conscious is, accordingly, that of active intercourse. This relation is elaborated, although often in language which is pictorial rather than literal and precise.

By "some uprush of collective forces" a gentle and reasonable being can be transformed into a maniac or a savage beast; indeed "we are always living upon a volcano" with no known human means of protection against an outburst (16). "We may assume," Jung says, "that human personality consists . . . of consciousness and . . . of an indefinite large hinterland of unconscious psyche" (47). But again, it is not assumption but fact—"the fact that an individual consciousness is based upon and surrounded by an indefinitely extended unconsciousness psyche" (99). "The psyche reaches so far beyond the boundary line of consciousness that the latter could be easily compared to an island in the ocean" (102)—the island "small and narrow, the ocean immensely wide and deep" (102). Jung indeed is "not at all certain whether the unconscious mind is merely *my* mind" (46). At times he believes such an appropriation of it to be inaccurate (47). Indeed when he uses the figure of an island in the ocean, this suggests that the unconscious may, in a sense, be shared by many conscious minds, as in actual geography many islands may all be in the one ocean.

Now to the ungracious work of doubt and appraisal—some of which is already done, for in the very attempt to bring together the scattered parts of some thought, to let the reader see it as a whole, the parts have at times refused to fit together. One is left in doubt because Jung, it may be, is himself in doubt, and puts down his thought on the selfsame matter now with hesitation and now with assurance.

Psychologists trained in the laboratory and in statistics will, I fear, be left unsatisfied with Jung's account of the origin of the image-symbols he calls archetypes, particularly with his conclusion that they are natural symbols which recur spontaneously in individuals, who neither influence one another directly nor are influenced by a common social inheritance. His patients, he is convinced by careful investigation, he says, were completely ignorant of what might have come from such influences. The details of this investigation are not given in the present volume, and one is left to doubt whether all possible sources of knowledge by the patients—from long forgotten reading or hearing have been or could be completely examined and excluded. One may be the more cautious because Jung's theory of the origin of these image-symbols goes beyond those fundamental psychological powers which, for Kant and others, have given to our experience its general framework of spatial and temporal relations: According to Jung, there are inborn tendencies to create fairly particular spatial patterns and to give to them a fairly definite and complex meaning by which they are highly significant religious symbols. Until other possible explanations are fully examined and excluded, one can accept all the facts observed by Jung regarding these symbols, and yet remain unready to accept his theory of their origin.

One may be especially unready to believe that the number four with its freight of associations should be counted as a natural symbol. One bears in mind that a considerable array of other numbers—one, two, three, five, seven, eight, ten, twelve, and higher—have each been freighted with significance for religion, morals, magic, science, and practical affairs. Cultural development would seem still to be our most promising source of explanation of each of these numerical ideas, of its image-representation, and of its significance as a symbol generally.

These doubts must not obscure the value of the book. It makes a welcome contribution to our store of observation of abnormal minds, by a very distinguished practitioner and leader in this region. Facts are stated, questions are raised, and answers are offered. The author pleads for nothing but a scientific scrutiny of some important and intricate observations. Jung's manner, while at times sharp against opposing ideas, is usually with dispassionate conviction, with now and then a sentence warm in its sympathy and appreciation. There will be many psychologists ready to study the book no less dispassionately and with no less appreciation.

University of California GEORGE M. STRATTON

Studies in Experimental Phonetics. By Giles Wilkeson Gray, H. Philip Constans, Wesley A. Wiksell, William Hobart Sallee, and Harriett R. Idol. Baton Rouge. Louisiana State University Studies, Number 27, 1936. Pp. 164.

Teachers of speech and voice have emphasized for many years the importance of breathing in voice production. Unfortunately, however, much of the material on this subject which has been included in textbooks of speech has been incomplete, confusing, and, all too frequently, contradictory. Dr. Gray's series of studies on breathing in speech attempts to determine how much of the current literature on this subject is based upon fact and how much upon fancy. Few monographs in the field of speech have accomplished their objective as thoroughly.

The monograph embraces six studies. Mr. Constans shows that the *effusive* and *explosive* "forms of force" which are traditionally used to describe different types of breathing are, as a matter of fact, produced by the action of the articulatory, rather than the respiratory mechanism. Training in breathing as such, therefore, can have little, if any, effect upon these "forms of force." Rather, they may be thought of as terms which describe the type of *articulation* which the speaker uses, not the type of breathing.

Mr. Wiksell, who studied respiration in relation to the intensity of the vocal tones in speech, concluded that there is no "individual relationship between the type of breathing (abdominal versus thoracic) and the intensity of the voice." This is in sharp contradiction to statements found in numerous texts, *i.e.* that the abdominal breather has a more intense voice than the thoracic breather.

Mr. Sallee, studying respiration in relation to audibility of speech, concluded that there is little, if any, relationship between depth of inhalation and audiblity and, further, that there seems to be "no evidence that any one particular type of respiration is superior to any other in relation to audibility, either for individuals or for groups."

Dr. Gray in his investigation of regional predominance (abdominal, medial, or thoracic) concluded that "it is impossible . . . to predict from the type of respiration, or the degree of regional predominance, what any given person's voice is or ought to be, in the particular type of utterance with which these studies were concerned."

Miss Idol, from a study of respiration in relation to speech characteristics, concluded that "no measurable aspect of casual breathing or of breathing during phonation has been found which *per se* affects these vocal characteristics which were considered, namely, audibility, quality, articulation, functional efficiency, general control, and control of force."

Mr. Wiksell, in his study of controlled and uncontrolled types of breathing (an

abridgment of his doctoral dissertation) removes still more misconceptions about the relative importance in speech of different types of breathing. His results in general indicate that the type of breathing, whether abdominal, thoracic, or medial, has little effect upon any measurable aspects of speech or voice. It is more important to have an adequate quantity of air available than to have some air available from any specific functioning of the breathing mechanism, *i.e.* if a subject is proficient in getting a fairly large average volume of breath movement through the use of uncontrolled breathing [whatever type he is accustomed to], he will find it of no advantage, and it may even be a disadvantage, to develop the use of a controlled thoracic inflation, and . . . he will be even less successful in increasing the volume of breath movement by using an abdominal type of respiration."

The above summaries are only brief—and more or less randomly selected—highlights from the wealth of experimental data packed into the 164 pages of this monograph. Although the studies in general are largely of a negative nature and may, therefore, not be received too readily by the speech profession at large, it is safe to say that eventually Dr. Gray's work on breathing will have the same effect upon the traditional notions about the rôle of breathing in voice production that scientific studies on personality traits have had upon phrenology.

Purdue University JOSEPH TIFFIN

Problems of Psychology. By H. J. GRUENDER. Milwaukee, Bruce Publishing Co., 1937. Pp. xi, 209.

This book was written as a text for undergraduate courses in the "science and philosophy of our rational life" (p. v). It is intended to serve as a supplement to the author's *Experimental Psychology* and was written because of his view that psychology to become a real science instead of an empirical one must enquire into the ultimate sources of consciousness and thus become philosophical as well as scientific.

He accepts the scholastic interpretation of Aristotle's philosophy given by Thomas Aquinas and in terms of its postulates and hypotheses critically analyzes the following problems: the image theory of thought as interpreted by Titchener; the process of abstraction; free will and modern difficulties against free will; the problem of personal identity and the localization of thought. He shows clearly that the logical conclusions to which sensory associationism leads are untenable and explains the Würzburg hypothesis of imageless thought as consistent with the scholastic doctrine. He rejects Berkeley's view that generalizing abstraction is impossible because it is inconsistent with the scholastic interpretation of thought and the experimental findings of Kuelpe and other investigators of the Würzburg school.

Some of the arguments against free will, he thinks, are due to a misunderstanding about the theory of free will. The scholastic theory of free will does not assume *absolute indeterminism;* that is, that man is not influenced by motives, acquired tendencies, habits, his environment, etc. Free will is defined as freedom of choice between two alternative courses of action by the *active interposition of the Ego* (p. 70). This view is presented by syllogistic reasoning which, because of the abbreviations used in presenting it, becomes very abstract and it would be difficult for a student who had no knowledge of philosophy to follow. The scholastic doctrine explains the Ego not as a duality composed of body and soul but as . . . "the

composition of man arising from the union of two incomplete substances" (p. 137). In considering the problem of personal identity, he has given a concise statement of the hypotheses in regard to the mind-body problem advanced by the various schools of modern materialism, all of which he rejects for the Aristotelian concept. The presentation of this problem would have been more effective if the theories had been given with more reference to those who have advocated them and to the historical setting out of which they developed. The argument for the scholastic doctrine is concluded with the explanation that thought and volition are spiritual activities and that the soul of man though conditioned by the body may exist apart from it and so constitutes the noblest part of man.

There is a need at the present for psychologists to formulate more clearly the postulates upon which their investigations and explanations of behavior are based, but many psychologists and philosophers will think that a systematic consideration and evaluation of the validity of those postulates is the problem of the philosopher and not of the psychologist. It is the writer's view that the first course in philosophy should give a student a more general orientation into the field of philosophy with less emphasis on establishing the validity of the concepts of one philosophical system. The book, if supplemented by much reading in the history of both philosophy and psychology, would undoubtedly help those who plan to major in psychology clarify their own conceptions of the metaphysical problems considered.

Georgia State College for Women EURI BELLE BOLTON

Causes of Crime. Biological Theories in the United States, 1800-1915. By ARTHUR E. FINK. Philadelphia, University of Pennsylvania Press, 1938. Pp. x, 309.

The early history of American criminology, penology and theories of crime is more or less hidden in chance articles in biology, medicine, anthropology and psychology. The men making these various contributions were usually busy men engaged in other fields, chiefly medicine. While their contributions are valuable, very interesting historically and often quite essential to any present understanding of the ideology in modern criminology, they have been more or less scattered so that to the average reader in penology there has been a distinct gap in the early literature. We tend to read the European writers, as Lombroso, and to jump from there to the more recent contributions in the United States. It is also usually customary for writers in penology or criminology to refer to the European sources and to neglect the American sources.

What Dr. Fink has done is to carefully edit and digest the essential ideological factors—biological, psychological, medical and social. His own statement as to the reason for the study are pertinent. "The conclusion," he writes, "is inevitable that the historical record of the early labors is bare because social historians have failed to probe into and interpret the nature and extent of these accomplishments. This study undertakes to meet this need." Dr. Fink has done a good job in editing and in simplifying. Anyone who has read the elaborate categories of Spurzheim with 35 sub-headings or Gall with 26 sub-headings knows the tendency for hopeless elaboration if one once gives way to the temptation. The author has carved a very clean-cut path of essentials and classified them under the following headings: Phrenology; Insanity; Moral Insanity (which is still very much alive as evidenced by the symposium in the British Journal of Medical Psychology a few years ago); the

part played by Alcohol and Drugs; Criminal Anthropology; Heredity, which is divided into a chapter in which the author deals with the theoretical side, followed by a study of a reëvaluation of the Jukes and the problem of Human Sterilization; and finally, Feeblemindedness.

The author brings this historical period (1800 to 1915) to a close with the introduction of the work of Goddard dealing with the application of the Binet test in this country and the publication of Healy's *Individual Delinquent.* Healy's contribution marks the turn of ideology from a hunt for stigmata and group causes to an emphasis upon individual difference and a vast interlocking directorate of causes of crime.

It hardly needs be said that the book might be very dangerous reading to an individual who read the contribution, let us say, on classificatory anatomy and faculty psychology and was unfamiliar with the critical views that have been added in recent years, such as Lashley's theory of equipotentiality.

The book is a valuable contribution and will save the teacher and the critical student of criminology hours of weary hunting for minor historical threads in less accessible books and references. It will give him the essential gist of a writer or a viewpoint with a minimum expenditure of energy.

University of Pittsburgh W. T. Root

Are We Movie Made? By Raymond Moley, New York, Macy-Masius, 1938. Pp. viii, 64.

This reviewer opened "Are We Movie Made" in full expectation of finding a layman's view of the movies, heavily illustrated by anecdote and worth perhaps a three-line review in this Journal. If others do the same, they may well share the writer's amazement at discovering that the monograph contains a sharply abbreviated but capable summary of Mortimer Adler's critique of the Payne Fund studies on the effects of motion pictures. The original report is made in Adler's "Art and Prudence."

Defending the thesis that "the scientific work that has been done (on the effects of movies) is of little or no practical value to the prudent man," Moley takes up Adler's criticism of each of the Payne Fund reports. The study of the movies as a condition of juvenile delinquency, by Blumer and Hauser, is severely criticized on the grounds of type of material used, of 'loading' these materials by biased instructions, and of drawing conclusions not warranted by their data. Peters' investigation of standards of conduct in the movies is taken to task for employing questionable standards of comparison and for making biased extrapolations from actual results. Dale's report on the content of moving pictures is attacked as arbitrary in classification, faulty in method of inspection, and as providing "discussion wholly unrelated to his elaborate and costly studies."

There is no point in carrying our summary further. Only three of the studies are accepted by these critics as representing "science properly used." The other studies are acridly portrayed as incompetent in technique, or as prejudiced in interpretation or both. Charters' summary is depicted as even less free from these sins of interpretation than the investigations he seeks to integrate, while Forman's "Our Movie Made Children" is flayed as a popularization in which conclusions outrun data by intolerable amounts. The concluding sections of the book present *verbatim*

Adler's conclusions together with an exposition of his thesis that "the first obligation of the moralist is so to train men that they will be able to receive any work of art without harm; only if he fail in that will he have to concern himself with regulating the reception of works of art."

The criticisms of the Payne fund studies are worthy of an answer from the original investigators. If they have been guilty of even a small proportion of the abuses of which they stand accused, psychology will gain by a public investigation of the charges. Until such an investigation has been made, no psychologist who reads this book will be likely to be over-free in quoting the conclusions under discussion.

The careful reader may share this reviewer's regret that the author has committed the reformer's error of trying to paint a uniformly black picture; in seeking to maintain his critical position he has been guilty of straining at certain methodological gnats. One regrets, too, that the acid of criticism so freely applied to the works of others should change to honey whenever Mr. Moley considers Adler's contributions. Others may err, but when Adler speaks "the sharp edge of analytic reason moves through the half truths and prejudicial conclusions of these studies like a keen scythe in a clump of weeds." Such sophomoric adulation ill befits the critical air of the essay. Nor is one overly impressed with the 'solution' over which Mr. Moley labors so lovingly. One feels that it might work in a world of philosophers but that it has little bearing on actual children going to actual movies. So faithfully and so unquestioningly does Mr. Moley label the movies as an art that one wonders how frequently he has gone to observe the nature of run-of-the-mine movies.

In sum, the volume presents in brief compass a critique of methodology and of interpretation in a well-known series of social studies. The critcisms are serious enough to warrant an answer, and the negative contribution is correspondingly large. Its positive contribution and its undeviating aura of hero-worship seem less likely to bulk large in later studies of the effects of moving pictures.

University of Maryland JOHN G. JENKINS

Psychological Aspects of Business. By E. K. STRONG, JR. New York, McGraw-Hill Book Co., 1938. Pp. x, 629.

Those who are familiar with Strong's *Psychology of Advertising and Selling,* published in 1925, will find in this new volume a great many threads of continuity. Not that it is a rehash of the earlier book, for new chapters have been added and the old chapters have been thoroughly revised. The continuity is rather in point of view. In 1925, Strong broke sharply with the traditional, static point of view to write a dynamic psychology of advertising in terms of human wants; in 1938 a study of wants still provides the framework for his treatment.

The volume opens with two sections which set the stage for later chapters by analyzing the acts of buying and selling in terms of wants, obstacles, and solutions. The same sections contain a discussion in general terms of how man comes to have and to attempt to satisfy his wants. A section on "Principles of influencing others" treats such varied topics as consumer research, motivation, and propaganda—all of which are interpreted in terms of wants and solutions of wants. The fourth major section of the volume is concerned with the tactics of selling; here the process of selling is followed from the initial prospecting through to the closing of the

sale. A section headed "Research techniques" proves to be an elastic receptacle for such disparate items as questionnaires, rating scales, job analyses, and safety programs. The volume concludes with a section on employer-employee relationships, including the wants of both groups and problems of leadership and morale.

The volume is rich in documentation and there is much evidence that Strong has read widely and thoughtfully. There are many references to his personal contacts with the several fields and a pleasing lack of vague generalizations made in the absence of direct knowledge of conditions. The author writes with conviction and in simple literary style. His case-material has been carefully selected and his sales-interviews have obviously been tempered by actual use.

The shortcomings of the volume suggest that it was written without the benefit of any considerable amount of extra-mural criticism. The author's definition of *want,* for example, is veiled in ambiguity which might have been eliminated if other psychologists had worked over the manuscript before it was published. Thus we find initially that a want "is a drive—a physiological mechanism," only to discover a few pages later that it "is a sensation," while a third section informs us that a want "is comparable to mental set . . . purpose is another synonym." Because a dynamic interpretation is so patently required by current workers in this field, one regrets that Strong was not required to clarify this basic concept before final publication.

Criticism before the manuscript was printed might also have pointed out to the author the irritating frequency with which he employs an unjustified definite article. Surely his exposition would have gained both in force and in dignity if he had spoken of "three factors" or "four conditions," rather than *"the* three factors" or *"the* four conditions." Such criticism might also have called the author's attention to his tendency to relapse into a taxonomic, *a priori* discussion of advertisements in the same volume that contains his review of the very market research methods which have cast suspicion upon this earlier way of 'analyzing' advertisements.

The volume as it stands is a stimulating presentation of the facts and methods in the wide field afforded by psychological problems in business and industry. The better parts of the exposition will take their stand with the best that has been written in this field. The looser parts are reminiscent of the commercial sales manual in language and in their comforting assurance that everything will turn out as predicted. The excellence of the former makes one regret that the latter were not more thoroughly passed through the alembic of alert—if friendly—criticism.

University of Maryland JOHN G. JENKINS

The Development of Children's Concepts of Causal Relations. By JEAN MARQUIS DEUTSCHE. Instit. Child Welfare, Monog. No. 13, Univ. Minn. Press, 1937. Pp. vi, 104.

Employing a group-testing technique, Dr. Deutsche secured the written explanations of various phenomena by 732 children between the ages of 8 to 16 yr. There were 11 questions with demonstration, and 12 questions without demonstration. The answers were subjected to exhaustive analyses, both quantitative and qualitative, and a wealth of results was secured.

The quantitative method used in analyzing the data, however, is reported un-

clearly and, as we learned in correspondence with the author, incorrectly. As far as we have been able to understand it, the method was as follows. The children's answers to the questions were evaluated in terms of their adequacy as scientific explanations by 13 graduate students, who were given a set of typical answers to each question plus explanations for each phenomenon by a physicist. The judges rated the answers on a 7-point scale each point of which was carefully defined. So much is clear, but from here on the exposition of the method is inadequate and erroneous. The raw median rating values thus obtained were then, apparently for purposes of weighting, expressed as percentages of the perfect score, *i.e.* of 7. These percentages were then converted into their corresponding sigma values and the sigma values were then *multiplied* by 2 (as we were informed by letter from the author—the monograph reports "divided by 2"); 46 was subtracted from these results, and the remainders were divided by 10. The nearest whole numbers were taken as the "final quantified scores."

Assuming a set of raw median rating values varying between 0 and 7, the reviewer carried through the calculations indicated. The resulting scores were all negative, and inversely related to the raw scores, and, if the nearest whole numbers were taken, the final scores obtained from such raw scores as 1, 3.5 and 5 were all —5. Dr. Deutsche's final results were all positive, ranging from 0 to 8, with the most adequate answers securing the highest scores. In every particular, then, our calculations ended with results different from those of the study. It is difficult to see how the author arrived at her values; and it is even more difficult to find a justification for her method.

In the light of these inconsistencies and errors, it is difficult to discuss the quantitative results. Let is suffice, then, merely to indicate that the author investigated age and sex trends; and the relationship of the "quantified scores" to socio-economic status, intelligence and school grade. Evidence was found to suggest that training and experience as acquired in the schools are the most important factors of those studied in determining the ability of the children to answer the questions on the tests.

In the qualitative analysis, three raters independently classified the answers according to Piaget's division of 'causal thinking' into 17 types. The resulting disagreement made it clear that Piaget's classification was inadequate, dependent upon the individual judge, and that certain of his types are very specific to a certain kind of question. A group conference was necessary before a unanimous classification was reached.

Analysis of the five 'types' which were found in "fairly high percentages," *i.e.* from approximately 6% and up, led the author to suggest that "causality does not develop in definite stages, with types of response characteristic of one age and not another." The kind and level of the answer seems to depend in great part upon the question asked, and not upon the age of the child.

No significant age-differences were found in the number of materialistic or non-materialistic explanations given. When the materialistic answers were subdivided into five categories, however, a direct relationship between increasing age and the percentage of answers using cause-and-effect relationships was found.

The answers secured in this study were compared with the results of other investigations, in which similar questions had been used. The answers from investigation

to investigation were highly similar. No evidence was found to support "Piaget's classification of the answers to specific questions according to stages of development." In common with all investigators who have repeated Piaget's questions, there was found "a much higher percentage of naturalistic explanations . . . than Piaget reports," and prelogical answers were "conspicuous by their rarity."

There is an introductory chapter containing a review of the relevant literature; and a chapter analyzing the number of words used. The chapter on "Interpretation of Findings" includes an extensive summary. At the end of each chapter, too, there are convenient and good summaries. There is an index, a bibliography (42 ref.), and many graphs and tables.

Cornell University JOHN IRVING LACEY

The Use of Tests and Rating Devices in the Appraisal of Personality. By ARTHUR E. TRAXLER. New York, Educational Records Bureau, Bull. No. 23, 1938. Pp. 80.

"This bulletin has been prepared to present a brief summary of the field of personality measurement, together with a description of some of the leading contemporary tests of personality and a summary of research dealing with them. The purpose is to provide a simple, non-technical manual on personality measurement for teachers in service who may contemplate the use of personality tests in their work, but who are too busy to survey the whole field of personality evaluation in order to select the tests suited to their situation."

Traxler states that there are between 400 to 500 published personality tests. Of these, 45 are considered among the most useful and are described in detail. His bibliography includes 182 titles, all of which are relevant to the field of personality and of interest particularly to the student who wishes to pursue the subject beyond the limits set forth in the bulletin. Critical comments of personality test-procedures are to be found in all the chapters, but principally in the Introduction and in Chapter V.

In his evaluation of the usefulness of personality tests, Traxler is constructively critical. He definitely points out that most personality tests have been hastily put together and lack validity because of the absence of validating criteria. For these and other reasons, personality tests are seriously limited for purposes of individual diagnosis. Yet, the author is of the opinion that a "large number of good studies dealing with certain personality tests are now available." Moreover, he states that the field manifests satisfactory symptoms of progress in that the present research workers are using improved statistical techniques, are ingenious in the development of new procedures, and are viewing their results with healthy scepticism.

The limitations, nevertheless, remain. One of the more significant is the fact that an unstable individual, may, in a test situation, manifest characteristics typical of him—thus destroying the validity of the data. In any event, the usefulness of a personality schedule, like an intelligence test, depends largely upon the skill with which the psychologist interprets results from the standpoint of the clinical history. It is the psychologist's task to discriminate between the significant and the trivial. In his hands, psychometric measurements, even though imperfect, throw a great deal of light on practical psychological problems and their solutions.

The Training School ANTHONY J. MITRANO
Vineland, N.J.

Psychology in Everyday Living. By F. F. POWERS, T. R. MCCONNELL, W. C. TROW, B. V. MOORE, and C. E. SKINNER. Boston, Heath, 1938. Pp. x, 511.

In the first four chapters of this book one encounters a naïveté seldom found in modern psychology texts. With a few exceptions the remaining chapters must be rated among the best to appear in books attempting to relate psychology to everyday life.

The first four chapters are characterized, in the main, by headings and subheadings with a minimum of enlightening exposition. Moreover, the exposition is often superficial, abstruse, or quoted or paraphrased from other elementary texts. To illustrate: Thought is discussed under the heading, "Difficult problems of psychology" and the subheading, "The nature of thought." The entire first paragraph follows: "Are there such things as thoughts? Does the fact that nobody has ever seen a thought prove that they do not exist? Can thoughts be transferred without sensory intervention?" The remaining short paragraph indicates that attempts to transmit thought by telepathic means have led to ambiguous results. With the exception of a brief consideration of reflective and creative thinking which was written by another author and appears near the end of the book, the above constitutes this book's contribution on the nature of thought. In the third chapter, entitled "Foundations of behavior," one finds something approximating a scrapbook of quotations and paraphrased statements from other texts. The uninitiated might gather that Hunter is an authority on the structure and functioning of the pyramidal tracts, the limits of tonal sensitivity, and the interdependence of taste and smell; Murphy on the functioning of the endocrines; Lund on polarization in neural activity; and so on. As illustrative of this point and also of abstruseness in exposition, take the following example: Of tonal limits, the author says, "Hunter gives the range of ability to receive and transmit sound to the brain for the receptors of the human ear from eighteen complete vibrations per second, to a frequency of 15,000 to 22,000 complete vibrations per second." In the following chapter we learn that "Personality is that portion of the totality of an individual's conduct to which others respond by emotionalized evaluation or the evaluation itself."

Such writing characterizes less than a fourth of the book. As already indicated, the remaining chapters are, with a few exceptions, highly praiseworthy. In a section entitled "Understanding ourselves" are to be found excellent chapters on the psychology of adjustment, human motivation, intelligence and special aptitudes, and individual differences. These discussions are not only written in an interesting style, but they take cognizance of much recent original research. Two other exceptional chapters, in a section on "Adjustment to college and life," concern vocational choice and psychological factors in successful marriage. In a section dealing with "Learning and study" are chapters on the general nature of learning, acquiring knowledge and skill, the acquisition of attitudes, learning to study, and problem solving. These constitute a brief, but sound and well-written summary of present knowledge concerning the learning process. The final section of this volume introduces the reader to various applications of psychology. Business and industry, law, politics, religion, medicine, and education are considered. Each chapter contains questions for further study and a list of references. A long list of references to books on various aspects of phychology appears at the end of the volume.

In the reviewer's opinion, this book will find its greatest usefulness as a supple-

mentary rather than a main text. It is notably weak on sensory processes, neural functions, emotion, thinking, perception, and methodology. If the course is a brief one for students who intend to do no further work in psychology, the chapters mentioned in the above paragraph as providing an excellent survey of psychological contributions to everyday living may serve as an adequate substitute for the more orthodox treatise.

Vanderbilt University NORMAN L. MUNN

Electrical Signs of Nervous Activity. By JOSEPH ERLANGER and HERBERT S. GASSER. Philadelphia, University of Pennsylvania Press, 1937. Pp. x, 221.

This monograph incorporates the lectures delivered by its authors upon invitation of the Eldridge Reeves Johnson Foundation, and is designed to review and evaluate the actual status of nervous electro-physiology. As such it is particularly useful for the psychologist who wishes to gain an authoritative and intelligible account of those neural events which underlie his own problems of behavior. In the first three chapters Erlanger analyzes the action-potential of nerve in its various phases (alpha, beta, gamma, delta, B, C) and its relation to fiber-diameter, speed of conduction and amplitude of potential. In the later chapters, Gasser reviews the correlations of these potentials with heat-production, metabolism and excitability (refractory, supernormal, and sub-normal periods). While some of this material may prove to be rather technical for most psychologists, the whole treatment should be carefully examined by every student of behavior. It cannot be too highly recommended for all who wish a modern and lucid account of the mechanisms which underlie the simpler modes of response.

Recent studies upon conditioning in the spinal and curarized preparation clearly demonstrate that the topics examined in this book must be more seriously considered by the active investigator than has been true heretofore. Instead of contenting ourselves with the gross (intact) animal, we must analyze conditioning in terms of the concepts which have been so ingeniously and laboriously developed by the 'axonologists.' The following preliminary observations in the Rochester laboratory may serve to illustrate this contention. (1) 'Conditioning' in the spinal preparation begins with certain fibers (presumably the larger and faster) and then may or may not spread to other fibers. (2) Under curarization, some fibers (the smaller and slower?) cease early, leaving the others still active; these in turn fail as drug-effects increase. (3) In the spinal preparation, the same fibers which persist longest under curare are as a rule the only ones which show evidence of 'conditioning.'

These few observations indicate that we can no longer work with the gross units of 'nerve' and 'muscle' in our own field; we must begin to utilize the concepts of electro-physiology in the analysis of behavior.

University of Rochester E. A. CULLER

Practice in Preschool Education. By RUTH UPDEGRAFF and HELEN C. DAWE, EVALINE E. FALES, BERNICE E. STORMES, MARY G. OLIVER. New York and London, McGraw-Hill Book Co., 1937. Pp xvi, 408.

Dr. Updegraff and the head teachers in the Preschool Laboratories of the State University of Iowa here present a clear account of the aims and techniques of one of America's first and most influential centers of preschool education and research. The

authors begin by describing typical days in the 2-, 3-, 4-, and 5-yr.-old groups, noting both children's and teachers' behavior; subsequent sections detail the child's introduction to the preschool, the manner of providing health and physical care, and provisions for motor activity, eating, resting and elimination. Of special interest to psychologists are five chapters on intellectual development: The General Environment, Books and Pictures as Sources of Information, Nature Study, Social Studies, Language and Speech, and a single chapter entitled Guiding the Child's Personality Development and Social Behavior; a chapter of primarily educational import discusses æsthetic development; the final chapter is devoted to collateral subjects: parents, staff, the use of the laboratories for observation and research, and the training of teachers. The appendix provides a veritable manual of teaching equipment and technique, rating and record forms, as well as bibliographies of children's books and of literature on preschool education.

Enough has been said to indicate that this book will be invaluable to preschool teachers and administrators and to many parents. Of more importance to readers is the function which this volume, with its virtue of taking nothing for granted, can play for research workers in the field. It provides a clearcut and unambiguous background of what happens to a child outside of the experimental situations in a typical, good American nursery school. Thus, of interest to many psychologists, is the expression of aims in social adjustments. Such 'rules' as the determination of ownership by use indicate sharply the kind of cultural conflict that the nursery school must take account of and in whose creation it must inevitably share. Equally clearly presented is the rather 'cool' emotional tone that is set by the teachers, which has been criticized by some psychologists and psychiatrists who consider that nursery school children are at a point of development where real 'mother-substitutes' are necessary to them. Whatever one's views on such topics data for their consideration are explicitly and objectively presented. The publishers have provided a splendid format for this useful book.

Sarah Lawrence College LAWRENCE JOSEPH STONE

Modern Society and Mental Disease. By CARNEY LANDIS and JAMES D. PAGE. New York, Farrar and Rinehart, 1938. Pp. vi, 190.

In a very compact little volume, the authors have enclosed a mass of material ranging all the way from the definition and classification of mental disease, to compilations and interpretations of statistics of mental disease in relation to age, marital condition, urbanization and economic conditions. In addition to all of these, they devote 20 pages to heredity, sterilization, marriage and eugenics. The book is a landmark on the road from clinical impressionism to science in the field of mental disease.

The authors are particularly to be congratulated upon their insight into the variables which must be considered in the interpretation of mental disease statistics. Certainly it will do much to correct many of the current misconceptions which we find today in much of the literature in this field.

Although the volume is encyclopedic and critical, it has many obvious flaws. Its encyclopedism does not signify that it brings together all of the evidence either for or against certain statistical problems in the field of mental disease. The criticalness of the volume is also occasionally marred by certain interpretations of data; for

example, that dementia præcox and manic-depressive psychosis have a hereditary basis because "the relatives of dementia præcox patients succumb more frequently to dementia præcox than do other members of the general population." Similar evidence has too frequently been used to "prove" the influence of environment and just as categorically one might question his conclusions regarding the effects of depressions upon hospitalization when based upon one economic depression (the current, or last one, since we are told it ended in June, 1932). Further, a question of technique might be raised as to whether the best way of studying such effects is to compare first admissions in the depression with those in the "pre-plus-post" depression period.

Whatever specific criticism such as the forementioned may be directed against the book, it is a distinct contribution to the field of mental disease whether viewed from a psychological, psychiatric or politico-social point of view. It is so compactly written that it is not easy to read but it is eminently worth studying.

University of Newark FREDERICK J. GAUDET

Indianer-Rassen und Vergangene Kulturen. By R. N. WEGNER. Stuttgart, Ferdinand Enke, 1934. Pp. 320.

This is, at least partly, a report of travels among natives of South America as far away from civilization as the author was able to make contact with them. The book contains a large number of very good illustrations of the people, their art and their social life. The life of many of these tribes is extraordinarily primitive. The weaving of textiles is virtually unknown. Cottonstrings, to make a hammock, *e.g.* are in a few places near the middle of their length tied together with cross strings, instead of weaving. Making fire by friction is an art used by occasional "artists" only. In general, fire is eternally kept up, and glowing wood, carefully wrapped, is carried along even on journeys. Clothing is unknown, refused, despised. The arrows used for hunting and war are often ten feet long, and the bows, made of palmwood, are very powerful and longer than mansize. Medical remedies are virtually unknown. A small amount of pottery is used, but for tobacco smoking rather than for eating and drinking. There are plenty of other tribes, however, which represent every shade of aboriginal civilization and of Mixed-European culture.

The author points out the remarkable fact that the cinchona plants the medical value of which became known to the Spaniards in the early part of the seventeenth century, are nowhere cultivated in South America, whereas they were introduced into Java for cultivation by the Dutch government through the aid of the German botanist Hasskarl in the middle of the nineteenth century, and spread thence over East-India.

The Indian permits little children to play with sharp knives and with fire; and the little ones, from a distance watched lovingly by their parents, soon learn to handle such dangerous things harmlessly. The Indian does not understand the European parent's cruel tendency to keep such things away from little children. Likewise two or three year old little children have already learned how to save themselves without hesitation if the narrow dugout canoe capsizes.

More than two hundred varieties of the potato are known to the highland Indian. Where the climate is too cold to raise maize, it is the chief food on which he

depends. The woman of the highland Indian has no civil rights, is totally subject, a slave, to her husband. The llama is treated with the greatest consideration, and its comfort comes before that of the owner. Its flesh is eaten only when it naturally dies. Music is made by means of flutes and drums. Too much alcohol is one of the economic obstacles.

The later parts of the book describe the ancient civilizations of Peru and Bolivia, the life of the present Indians who are descendants of those civilizations, the life of the colonial Spaniards in the sixteenth and seventeenth centuries, the latter also by means of many traditional anecdotes. Finally the Mayan architecture in Yucatan and Guatemala is depicted in a number of well printed illustrations.

Coral Gables, Florida — MAX F. MEYER

Mental Health through Education. W. CARSON RYAN. New York, The Commonwealth Fund, 1938. Pp. ix, 315.

This book is an account of how much the movement for mental hygiene has permeated the whole educational process from teacher training institutions to teachers and administrators down to the pupils themselves. With mental health clearly in mind, Dr. Ryan was able to visit these various types of institutions and in this book is set down both the actual findings and his interpretation of their significance. Samples of higher institutions, of school systems and of classrooms whose activities show the influence of this point of view are presented. In general, too few school systems have caught the spirit of the movement because it means more than a few "core" courses, activity programs, etc., but rather a change in the atmosphere in which the whole educational process is carried on.

Progress towards mental health is being made. 'In-service' courses in mental hygiene, pre-service training institutions, and some of the newer experiments in higher education are samples of improvement at one level. Then, too, many school systems and classroom teachers are placing less emphasis on grades and promotions, recitations and home work, examinations and marks and more on balanced emotional development, right attitudes, and a well adjusted personality. Finally, child guidance clinics, the visiting teacher, and psychiatric service for schools while fairly frequent are greatly needed in large numbers.

The book is well written. It smacks, however, too much of the philosopher and propagandist and too little of the scientist. Statements are made about the desirability of change without an iota of evidence in its favor. If one could grant the fundamental assumptions concerning the development of mental health the rest of the book would be more acceptable. One is thus forced to conclude that this book by Dr. Ryan is a careful exposition of opinion and philosophy but not a book of science. It is more a book for the faithful than a treatise for the unbelievers.

University of North Carolina — A. M. JORDAN

How to Teach: A Text for Upper Grade and Secondary Teachers. By CLAUDE C. CRAWFORD. Los Angeles, Southern California School Book Depository, 1938. Pp. 512.

The author recommends among other things that the teacher should make notes of all good ideas and apt illustrations as he encounters them in books, lectures, and discussions, and that he should preserve these notes, together with his own comments and reactions, in a carefully classified file. The author has evidently practiced

what he preaches for many years, and this book is one of the outcomes. It is not a scholarly book in the usual sense, yet it reflects a lifetime of scholarship. It contains no tables, no diagrams, no graphs, and no pictures. Such experiments as are described are presented sketchily, and are used as illustrations rather than as foundations for the discussion. The illustrative stories are numerous, well chosen, and well told. The book abounds with mis-quotations and errors of fact, particularly in the illustrations, but its discussions are singularly free from fundamental errors, misinterpretations of the genuine significance of the results or research, and systematic biases.

The book is a mine of practical information and sensible hints. The idea of learning by doing, of teaching processes rather than principles, and "how to do it" rather than "which is what," is dominant throughout its pages; yet such words as "dynamic," "functional," and "organismic" are not overworked. The author is progressive and modern in his viewpoint without being too "progressive." In his preface he states, "I haven't made very much effort to use a dignified and conservative literary style because I have been writing to the collegiate crowd instead of to the professors." He means what he says. His book is easily the most interesting and readable one on its subject that has been published in recent years. If a good many of the suggestions offered seem to be such as should occur naturally to any bright teacher, we may perhaps be overestimating the number of teachers who are bright. If Gestalt psychology is worked overtime—"A unit [meaning a teaching unit] will be a whole, or a configuration, or a pattern, in the Gestalt sense," and "The Gestalt view of psychology puts tremendous emphasis on the fact that details have meaning only as parts of a total larger pattern; yet much informational teaching is still done on the isolated basis"—why perhaps the end may justify the means.

Those instructors who feel that the course in Principles of Teaching should serve principally to illustrate the progress in recent years of the science of experimental education will condemn this book. A goodly number of those who feel that such a course should serve primarily to show alike the applications of educational psychology, educational philosophy, and old-fashioned horse sense to the modern classroom situation as it actually exists, will welcome it as an important addition to educational literature.

Alabama Polytechnic Institute EDWARD E. CURETON

Krankheit und Tod im Schicksal bedeutender Menschen. By ADOLF BRAUN. Stuttgart, Ferdinand Enke, 1937. Pp. 104.

Medizinisches aus der Weltliteratur von der Antike bis zur Gegenwart. By ADOLF BRAUN. Stuttgart, Ferdinand Enke, 1937. Pp. 152.

These two books seem to have resulted from the author's endeavor to write the first. The material collected which did not strictly fit into the first, the collector, it seems, then put into the second book. The chapters of the first are constituted by the causes of death, such as general senility, violence, cancer, etc. The material should make good practice reading for a degree candidate who has to fulfil a language requirement. The data presented are interesting. Here are a few examples:

Tolstoy, at the age of 82 years, decided to be a monk. On his way to the monastery, at a railway junction, he got chilled and died of pneumonia. Mendel, age 62 years, died of obesity, from which he suffered many years as the result of heredity aided by the excellence of the chef of his wealthy cloister in Brno, Moravia. Hutten, the

poet of the Reformation, knighted by the Emperor Maximilian, after suffering from syphilis during 17 years, died at the age of 35 in exile in Switzerland, leaving nothing but a writing pen and numerous debts. Zola, age 62, was killed by carbon-monoxide, after having closed, on a chilly night in Paris, the windows of his bedroom in order to keep warm. Mozart, age 35, very poor, died from a kidney disease, was buried in a blizzard in a potters field, with no possibility of identifying the place. His wife lived fifty years longer. Poe, age 40, tuberculous, died in delirium tremens. Koerner, famous Prussian poet-patriot-soldier, age 22, was shot 1813 by a captured French officer whom he insulted. Tizian painted two masterpieces at the age of 99 and was killed by the bubonic plague. The corpse of Sterne was stolen from the cemetery and by a mutual friend identified in the dissection room of the anatomy professor in Cambridge who had bought it and had already cut it up.

Large is the author's list of those to whose death a syphilitic infection made a contribution. He mentions among them Erasmus, Cellini, Mirabeau, Donizetti, Lenau, Nietzsche, Smetana, Lenin, Wilson. Many of those thus suffering were mentally active, even extravagantly so, till the very end, in spite of pronounced symptoms of another kind.

The second book has a less unitary aim; and its style is more rambling. Among the famous personalities who are discussed from the standpoint of inner medicine, prevail those of antiquity and of German nationality. But it, too, makes entertaining reading.

Coral Gables, Florida MAX F. MEYER

A Laboratory Manual in General Experimental Psychology. By NORMAN L. MUNN. Revised edition. New York, Prentice-Hall, 1938. Pp. viii, 286.

In this manual the author aims "to acquaint students with the experimental investigation of outstanding current psychological problems." The distribution of the 65 experiments by subject matter may be roughly classified as follows: nervous system and sensory organs, 3; motor and condition responses, 5; sensation and perception, 23; learning and memory, 13; animal, 5; feeling and emotion, 4. There are one or two experiments each on such topics as thinking, personality, aptitude, imagery, suggestion, testimony, and attitudes. Each experiment has an annotated bibliography. In the appendix are suggestions on equipment.

Certain criticisms of the manual suggest themselves. Although the author, in many instances, suggests alternate and less expensive equipment, a large number of the experiments call for apparatus which is not available in most laboratories. Because directions to the student are frequently meager, both as to experimental procedures and treatment of data, a heavy teaching load is placed upon the instructor. Many will question the advisability of basing the discussion questions, which accompany each experiment, principally upon the reference reading rather than upon data collected by the student. Norms or class data for comparison with individual results are not included. This text emphasizes content much more than method and leans heavily upon traditional psychology (*i.e.* much on sensation, perception, memory and little on personality, vocational and applied). The binding is such that the book will not lie open flat upon the table.

The reviewer believes that the most satisfactory laboratory manual is one that

emphasizes methodology, reflects to an adequate degree the contemporary trends in psychology, reduces the teaching load to a minimum, requires only simple apparatus, and is handy for the student to use. This manual cannot be rated as entirely adequate on any of these points.

Like most recent texts, this is the work-book type. The list of experiments is sufficiently numerous and varied so that the instructor has a wide range of choice. The book will find favor with those instructors who are able to select a pattern of experiments which is fitted to their laboratory facilities and objectives.

University of Minnesota MILES A. TINKER

Studi Sulle Precognizioni. By LEONE VIVANTE. Florence, Vallechi, 1937. Pp. 218.

Vivante began these studies of the phenomena of precognition because such phenomena appeared to him to contradict a doctrine of originality of thought to which he had always adhered. He wishes, he states, to investigate the genuineness of such phenomena before accepting conviction. He asks: "What roads must be followed in order to explain the phenomena of precognition?" He selects as his criteria the methods and principles of modern science.

A large share of the book is given over to a critical consideration of historical data and case studies of the last fifty years. Vivante draws rather generously from the works of Bozzano, Richet, F. W. H. Myers, and Osty. He reviews the more important theories of those who have published on the subject of precognition and also presents the traditional, popular interpretations of precognition as well as the interpretations of mediums. The final chapter is a discussion of the relationship between normal and abnormal phenomena.

Vivante is guarded in his evaluation, but suggests that phenomena of precognition are but aspects of behavior which have become exaggerated through retrospection; the manifestations are emphasized in disregard of the law of probability.

The book is carefully documented, scholarly in presentation, and worthy of translation. There are, however, some notable omissions. Murchison's *The Case For and Against Psychical Belief* is not cited nor is any reference made to the controversies raised by the Duke experiments in America.

The emphasis, although creditably psychological, is philosophical. Vivante comes to no hard and fast conclusions. He is neither "for" nor "against" the concept of precognition, preferring to urge that more research be conducted.

Rochester, New York ANTHONY J. MITRANO

Over Polariteit in de Zielkunde. By C A. J. HARTZFELD. Bussum, Van Dishoeck, 1937. Pp. 40.

This really valuable little study considers more generally the problem of polaric conception in psychology as it has become more and more applied in the last 30 years only to mention C. G. Jung, Kretschmer, and Janet. Hartzfeld brings up the question about the value of such kind of psychologies in general and sees it in the differentiation which becomes not so evident in unitaric viewpoints. The "double viewpoint" enlightens not only different members of psychic behaviour but gives too a better opportunity to value the totality and unity of psychic facts and psychic life itself.

Washington, D.C. ERNST HARMS

Durrell-Sullivan Reading Capacity and Achievement Tests. By DONALD D. DURRELL and HELEN BLAIR SULLIVAN. Yonkers-on-Hudson, N.Y., World Book Co., 1937.

These tests, designed for grades 3-6, are constructed on the assumption that reading disabilities are revealed by discrepancies between the understanding of spoken and of printed words. The first test, a Reading Capacity Test, seeks to discover the subject's potential reading ability. In this test the subject's vocabulary is checked by having him, in response to an oral question regarding 'word' meaning, select from a group of eight pictures the one that best answers the question. Following this part of the test, the subject's 'paragraph' meaning is checked by five questions in answer to each of which he is required to select one of three pictures—only one being correct.

The second test, a Reading Achievement Test, measures the subject's vocabulary by a five-item completion-test and his comprehension of 'paragraph' meaning by tests of his ability (1) to understand sentences, (2) to understand words in their context, (3) to note detail, (4) to grasp the central thought, and (5) to locate specific details of information.

By the means of age and grade norms given in the manual, it is possible to determine whether the child's deficiency is due to his lack of understanding of the spoken or printed word, to his lack of vocabulary, or to his lack of any of the five items measured by the reading achievement test.

Spelling tests and written recall tests are given as optional. High reliabilities were obtained by the authors in all the tests.

Ohio State University. ROBERT Y. WALKER

Love, Hate and Reparation. By MELANIE KLEIN and JOAN RIVIERE. London, Hogarth Press, 1937. Pp. vii, 119.

This second volume in the Psychoanalytical Epitome Series presents two lectures given by the authors in a course which described the emotional life of civilized men and women. Many of the ideas and impulses in the adult mind are traced to mental functions unconsciously remaining from infantile and childhood experiences.

The first lecture, by Riviere, is entitled, "Hate, Greed and Aggression." These mental attitudes are destructive, while love is unifying in nature: they stem from the great primary emotional patterns of hunger and biological love respectively. Later in life the projection of these inwardly felt emotions develops into a two-fold interest in other people. The former destructive group, centering around hate, seeks an external outlet for the discharge of our meaner traits, while the latter strives for objects of satisfaction.

The second lecture, on "Love, Guilt and Reparation," by Klein, stresses the contributions which love makes toward a more stable society from the early parental object to the friendships and marital partnership of maturity. Contentment and satisfaction thus gradually supplant the earlier feelings of guilt and resentment connected with frustrations of natural impulses.

The work is interestingly written but would have gained value in this direction if the two lectures had been recast in systemtically condensed form as one comprehensive essay on the subject.

Minneapolis, Minn. CHRISTIAN A. RUCKMICK

Recent Experiments in Psychology. By LELAND W. CRAFTS, THEODORE C. SCHNEIRLA, ELSA E. ROBINSON, and RALPH W. GILBERT. New York, McGraw-Hill Book Co., 1938. Pp. xiv, 417.

There are twenty-eight chapters in this book and each one is written around one or two clear-cut and, in the majority of instances, crucial experiments. The experiments are intensively treated and particular emphasis is placed on the exact procedure and apparatus employed. Many excellent figures of apparatus clarify verbal descriptions. Although the reader is given an intimate account of the operations performed by psychologists in the laboratory, each chapter also contains a careful statement of the general significance and practical importance of the experiments discussed. The reviewer is impressed by the skillful and dignified manner of exposition.

One could have asked for a wiser selection of topics in only three instances. The chapters on the relation between character traits and handwriting, the judgment of vocational aptitude and achievement from photographs and the representative and expressive effects of music are interesting and well set forth but they do not measure up to the general significance of the remaining twenty-five chapters. These chapters are all so skillfully handled that it is difficult to single out any one or a group for special commendation. The reviewer is gratified by the discussion of the migration of birds and salmon, Honzik's important experiment on sensory control of the maze habit, brain waves, occupational ability patterns, the conditioning of vasomotor responses, and the motor theory of thinking. All of the important concepts in elementary psychology are represented by one or more chapters.

Western Reserve University CALVIN S. HALL

The Abbe du Bos: His Advocacy of the Theory of Climate. By ARMIN HAJMAN KOLLER. Champaign, Illinois, Garrard Press, 1937. Pp. 128.

The theory that climate is responsible for art was a contribution of Abbe du Bos in the early 18th Century. The theory is now revived by Koller who presents the evidence and arguments for it. Even if one were to agree with the author that 'real art' is limited to a few countries of Western Europe (a proposition that many will challenge) it does not follow that the primary cause of that excellence is climate. Koller's conclusions are not based upon a comparison of the climates of Western Europe with other parts of the globe, nor upon a demonstration that climate has any specific physiological or psychological effect upon man. They are for the most part non sequitur.

Washington University JOHN PAUL NAFE

Het Domein Der Godsdienst: Psychologie. By F. J. TH. RUTTEN, Nijmegen, N.V. Centrale Drukkerij, 1937. Pp. 51.

The chairman of the department of psychology of the Roman Catholic University of Nijmegen surveys in this little treatise the most acknowledged theories of psychology of religion of our time as Girgensohn, Schjelderup, Thouless, Janet and C. G. Jung in their consideration of the psychology of worshipping. He distinguishes general, typological and genetic psychology of worshipping. His own is the characteristical Catholic one, nevertheless he maintains that the psychology of worshipping should be outside of the dogmatical confessions.

Washington, D.C. ERNST HARMS